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Darwin and Darwinism, Pure and Mixed. By P. Y. Alexander, M.A., LL.D.

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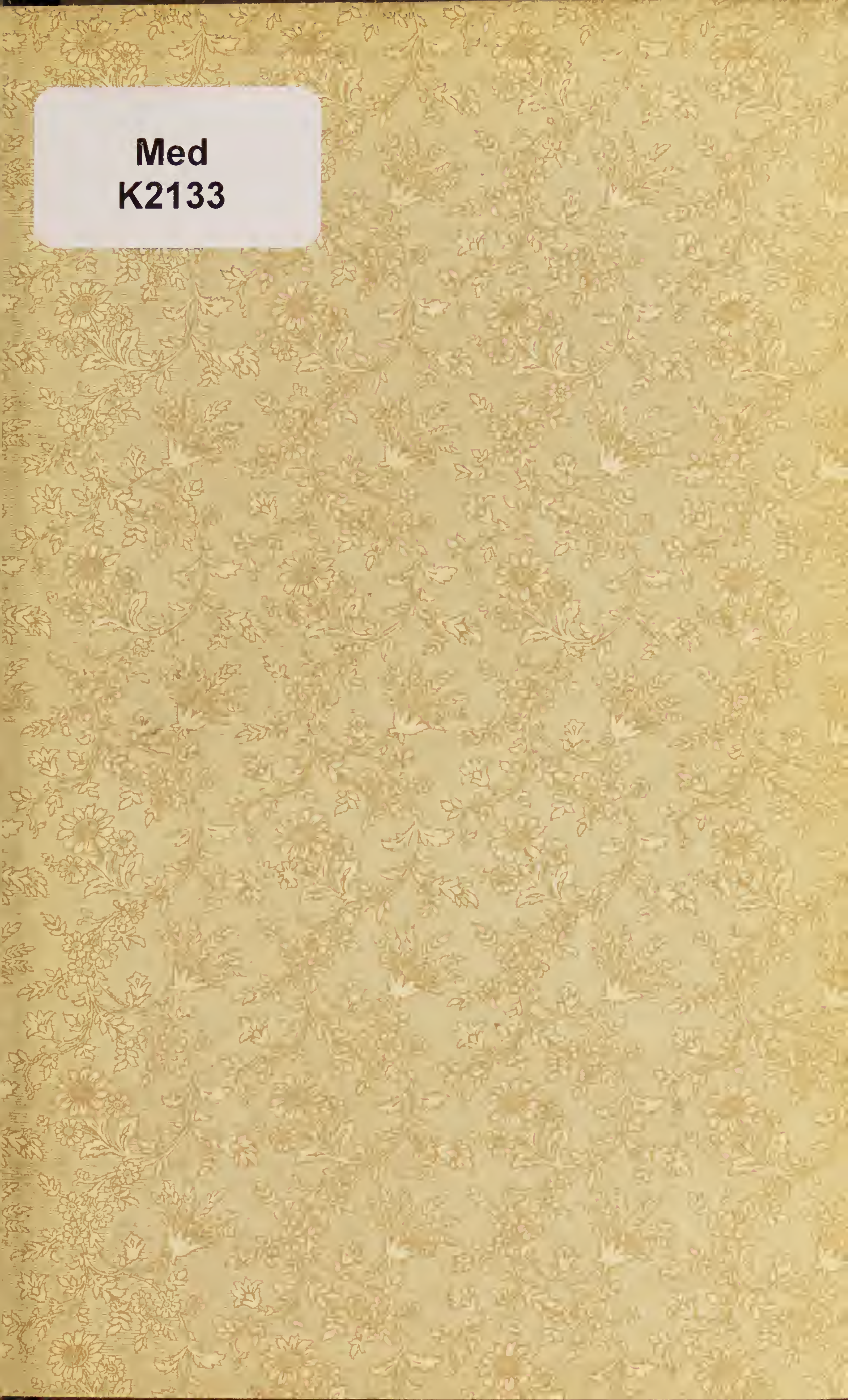

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DARWIN & DARWINISM

PURE AND MIXED,

A Criticism, with Some Suggestions.

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BY

P. Y. ALEXANDER, M.A., LL.D.

Author of "Heredity," "Parasitism," &c.

> "Look up to and reverence the Master, but let him not be constantly so close to your eye that you cannot see the sky and the sun or the stars, or the trees that wave their branches to invite you, and the birds in their beauteous plumage that flutter and would fain sing to you not far off. —*Dharmapitta, the Buddhist*, from the Sanskrit.

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PREFACE.

I HOPE my readers will not conclude from mere glances here and there at the pages of this book that I am impelled simply by a desire for a barren dialectic victory over certain followers of Mr. Darwin. What I am concerned for are the interests of science and of truth. I am fully convinced that nothing conflicts, or can conflict, more with these than the facile acceptance of formulas and phrases which a considerable thinker may have found, or fancied he found, of service to him in his investigations and researches. What is good as a *help*, more especially to the first user of it, may be very bad as a *master* to those that follow. I am sure of this, that phrases and terms, in effect repudiated by the master himself, ought not to be regarded with the reverence with which they are sometimes regarded. Mr. Darwin was one of the greatest observers and investigators the world has seen; he was as though gifted with a dozen pairs of eyes and also a dozen pairs of hands; but such an excess of power in one direction usually carries with it its own penalty—a curtailed degree of power in another. Mr. Darwin's capacities of thinking and drawing inferences from the immense masses of fact he had collected were not at all equal to his powers of observation, investigation and classification; my little effort will show that, wherever I have paid special attention to any department of Natural History or Natural Science, I am apt to find Mr. Darwin at fault—more especially in his generalisations; and I believe that I do a service to the coming generation, if not to the present generation, in citing some instances of this, if here and there it may seem that I fail to appreciate or to reverence as I ought the activities and the products of some of his successors. Certain lines only of discussion are opened here, others may be attended to later.

I would fain beseech for my book at least a careful reading and a fair judgment.

P. Y. ALEXANDER.

ARGUMENT OF THE BOOK.

1. The first point is to show how substantially Mr. Darwin altered his positions. The "Origin of Species," in its main ground, was completely superseded by the "Descent of Man" and later works, where Mr. Darwin asserted the extreme influence of *conditions* in the work of selection, which he had denied before; putting the whole thing on the *constitution* of animal or plant. Weismann, in his scheme, did precisely the same in trying to find a biological or purely scientific basis for Darwinian selection. But the germ-cells, or germ-plasm, he had to admit were not isolated from influence from body-cells, and that, indeed, only a nucleus of them was so insulated. Finally, in effect, he even surrendered that.

2. Mr. Darwin went for essential *slowness* as a necessity of nature. He said in "Origin": "Nature can never take any great and sudden leaps." When instances were presented to him of "sudden leaps," he tried to gloss it over, and always harked back on *slowness*.

3. While he did so, in spite of his notion of selection, he had to admit that many eggs and seeds were destroyed as good, or better, perhaps, than those preserved; so that he was fain to drop the phrase "natural *selection*" in favour of "natural *preservation*"—a much wider term, truly; for that things survive is proof they have been

preserved, of course. Some of his disciples have gone further, and, while pretending to stick by the "Origin of Species," actually go absolutely for "elimination," or, as they have it, "*killing off*"—which, looked at scientifically, lays the axe to the root of *selection* in any proper sense it can bear.

4. Mr. Darwin professed great regard for law, yet was constantly using terms and phrases such as *chance*, *happened*, *fortuitous*, *accident*, utterly alien to such a conception. His disciples, one and all, have here outdone the master—Mr. Romanes, more especially. "*Chance*," Mr. Darwin said, "is a combination of several systems of causes developed each in its own series independently of the others." This is shown to be absolute nonsense, which, indeed, it is.

5. "Specific types" are shown to be assumed in a most unscientific manner, and a new starting point then made with "individual fortuitous variations," which cannot be allowed by thorough thinking. No such arbitrary starting-point could exist in fact. Professor Ray Lankester's "non-significant congenital individual variations," shown to be a contradiction in terms—the *non-significant* an assumption to meet the theory—they are *significant* in extreme when verified; till they are so, they cannot be said to be more than supposition merely.

6. The cases of deer and their horns in growth, &c., so closely related to the sexual organs—the most sensitive of all organs, as Mr. Darwin at last discovered them to be—are found to contradict directly large sections of Mr. Darwin's theory, and if withdrawal of protective frontal weapons from the female (which, certainly, early female deer had as well as male deer), so as

to attest *secondary sexual characters*, then assuredly here is a case where *sexual selection* and *natural selection* are absolutely opposed to each other and cannot be reconciled on the facts.

7. Mr. Darwin's notion that "domestic animals, which have long been habituated to a regular and copious supply of food, *without* the labour of searching for it, are more fertile than the corresponding wild animals," shown by instance on instance to be *absurd*, opposed to the practice of all great breeders, and is, besides, physiologically impossible.

8. Mr. Darwin actually posits "uniformity of conditions" over long periods. He takes superficial appearance for reality and fact. There is not, never was, and never will be any such "uniformity of conditions." Were it so, you would have eras in which natural selection could not operate at all; and this, surely, is not what Mr. Darwin and the rest want to prove.

9. The appearance of man on islands or places hitherto unvisited of man causes immediately the beginning of vast changes. He cuts down trees, plants others, and disturbs whole classes of creatures, which, driven to migrate, affect intimately the conditions of whole classes of other creatures there, and so on and so on; suggesting a phase of selection which may be called *unconscious selection*, at which Mr. Darwin hardly glanced, yet it is doubtless the most pervasive and thorough, surpassing pure natural selection and artificial selection, and standing, as it were, between them.

10. The word "instinct" is used so loosely by Mr. Darwin and all these men of the school, that it were to be wished we could wholly get rid of that word.

Many instances are given—the lemmings, cuckoos, &c., &c., and Sir John Sebright and Dr. Russel Wallace quoted in support of this position.

11. Mr. Wollaston Hutton, in his recent book, is shown to be very self-contradictory and inefficient, more especially in his new doctrine of *use*, which actually harks back on the old theological conception.

PART I.

GENERAL VIEW OF MR. DARWIN'S POSITIONS.

MR. DARWIN'S GREAT CHANGES OF FRONT.

I HAVE had two most interesting letters—one from America and the other from India—regarding some general statements made at the close of an article in a magazine for February, 1899, on the "mound-building birds." I had said that Mr. Darwin was as much to blame for certain loosenesses of phrase as any of his disciples; that he was often contradictory and *shied* facing certain facts; and that here his followers only too closely follow him, making shibboleths of certain terms and phrases as though they represented realities, and treating "Natural Selection" and "Survival of the Fittest" as though they were something more than theories, or more than working hypotheses, whereas "they no more finally express the ultimate facts of Nature than former phrases did," and are no more than formulas, helpful, if properly used, towards the attainment of insight into Nature's ultimate laws. Mr. Darwin, as we shall see, came openly to confess this of his earlier term.

My American correspondent regards Darwin as "the greatest scientific mind the world has ever seen; accurate, patient, never proceeding beyond what facts observed justified, and never needing on any great point or principle to recall anything, or to confess an error." My Indian correspondent, again—a practical ornithologist, who has been studying Indian cuckoos, especially *Cuculus canorus* there and the koel—declares that so far as these provide facts to him he is inclined to think that I am right, and that Darwin was completely wrong in 8th chapter of "Origin of Species," about the whole question of cuckoo parasitism and the migratory instinct being the main agent in determining it, because, as he says, *Canorus* has abundant time in parts of India to do its own brooding, being there in the breeding ground certainly from six to seven months, whilst the koel is as parasitic as any cuckoo of them all, and is, like the great spotted cuckoo in Egypt, entirely resident.

Thus the "bane and antidote were both before me"—I had approval and praise on the one side; I had, on the other, severe criticism and opinions different from mine, and pressing questions asked of me. Well, now, to reply to my American correspondent fully and in detail would want a volume—nothing less; but I should much like him, in the first place, to look at Dr. Brooks's "Law of Heredity," which no doubt he can easily find, and turn up all the passages in the index referring to Darwin until he finds what Dr. Brooks has to say about Darwin's serious mistakes respecting "reciprocal hybrids" (p. 138), and about "a more deeply seated law" behind the action of "selection," which determines that males shall, as a rule, be more modified than females (p. 218). While he is doing that, let me briefly set down under six heads what I regarded as justifying me in writing as I did at the close of my article on "mound-building birds." From this I cannot recede till I have got some more explanations and fuller satisfaction about these.

I

That a man holding the high position Mr. Darwin did, with such unique opportunities for observation, experiment and study, should have blundered as he did as regards outward conditions as affecting modification is utterly inexplicable when viewed alongside his odd way of attracting to himself all the regard and trustfulness that apparent and unaffected candour could secure. Unlike many, if not most, scientific writers, he would begin with presenting, in all apparent sincerity and frankness, the drawbacks to his own theory, which he had just in outline presented. He was constantly putting certain cases, which if they could be found and verified, would ruin completely his own theory—careful generally, however, to protect himself by some unique qualification or exception or guarded phrase. But, assuredly, nothing of this protects him with regard to his putting on Natural Selection the whole tendency of evolution or survival of the fittest, and so completely ignoring outward condition, environment, summed up in climate, rainfall, food, &c., &c. Had entomologists not already told that colour, form, &c., in insects were modified, and immediately modified, in the most marked manner by that on which they were fed; that even in some cases the feeding of the larvæ projected effects modifying streaks of colour on the perfect insect, so that the failure of a certain plant in a given area changed the whole aspect of whole species; that the same species found on elevations were degrees darker than those found on plains—since there there was more rainfall, more humidity, and more shade; that many birds where the feline tribes abounded resorted to wholly different methods of nesting and brooding from what exactly the same species practised where there were no cats; that rabbits where the soil was such that they could not burrow lost the power of burrowing altogether, and that within a very short space of time; did he not know that cage-birds of all kinds if not allowed opportunity for a bath for a certain period were then actually frightened at water, and, though served with it and tempted to it, would not enter it; which suggests that long-continued droughts and the drying up of streams and pools over wide areas may modify the instinct of whole species in this respect. We have an instance yet more telling in this direction, quoted by a great Darwinian himself: "Mr. Spalding tried the experiment of keeping young ducks away from the water for several days after they were hatched;[1] on then bringing them to the pond they showed as much dislike to the water as young chickens would have done. Not being able to enter water shortly after leaving the nest, a water-bird was converted into another species.

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[1] Lewes on Instinct in Problems of Life and Mind, quoted by Romanes, "Ment. Evol." p. 171.

Then what about the way in which it is now undoubtedly demonstrated that soils are direct agents in deepening or lightening colour even in the case of artificially bred and artificially treated animals? Jersey cattle kept in Devonshire become some shades darker red, like the native breed; Somersetshire cattle are red, but become several shades lighter than the Devon, just in proportion to their distance from the Redsandstone. If this is the case with animals on which all influence that can be brought to bear by man to keep their marks and colours fixed for the sake of breed will be brought to bear, what must it be, by parity of reasoning, with wild animals in the case of their, under one necessity or another, moving from one area to another where the underlying stratas may be quite different? Clearly, the creation of what are really intermediate species. Mr. Darwin, when he wrote the "Origin of Species," would have been bound by his theory of regarding conditions "as absolutely subordinate" to have put this aside, though he might have recognised it when he came to write "Domestication."

Dr. A. Russel Wallace, in his essay on "The Tendency of Varieties to depart indefinitely from the Original Type," very effectively observes, of the assumption that *varieties* occurring in a state of nature are in all respects analogous to, or even identical with, those of domestic animals, and are governed by the same laws as regards their permanence and further variation, that it is altogether false, and his illustrations of the way in which the increase or even the existence of a variety is affected by risk of failure or great scarcity in the food supplies is to us wholly convincing. And in many cases is it too much to say that Mr. Darwin might well have guessed that changes of this sort in view of the preservation of the species, needed to be quick? as in the case of the hornbills and some allied species, else over whole regions they would have been exterminated by the sudden increase and excess of felines there. His desire to make out the rule of natural selection acting by extremest slowness undid him; for, in opposition to him, one of the most remarkable things is the suddenness with which such changes are *often* effected, as witness, for one thing, insect melanism in Yorkshire and the north of England, as well as in the Midlands, where whole species have become in a few years many shades darker. *Tephrosia biundularia* is proved definitely to have done so over whole districts; dark or darkish specimens were the great prizes as being rare less than a quarter-century ago, now light or cream-coloured specimens are the prizes, the dark ones being common. On slopes of high hills, as in Ceylon and India more particularly, the colours of the same species vary in a regular gradation, light or cream colour in the valleys, darkening more and more by height, till on the top they are almost black in some cases.

There is no doubt about it, but Mr. Darwin did not forecast that as he ought to have done; instead, he tried to go for extreme and almost invariable slowness, and for years and years closed his eyes to suggestions in this direction. He was compelled to see it later; but then his earlier books were all over the world, and he could not recall them; nor, indeed, modify them up to the mark that he himself recognised and confessed that they needed modification, revision and addition; and it is rather a startling thing to find that for the necessary corrections on his own books—not on trifles, mark you well, but on the most fundamental principles—you have got to go to his "Life and Letters" that were not published till after his death, and most probably will never be perused by very many of those who have allowed themselves to be influenced by his earlier writings. Even his most active disciples and proselytising followers do not seem always, when they are but too closely following his way of professedly humble but really bold guessing, with "must be's," "should be's," "may be's," "probably's," &c., to have in mind the master's *peccavis* and *caveats*. For to have these in mind is the most essential condition for any progress, any advance. Other writers have seen and dwelt on the "may be's." Prof. Fleeming Jenkin let in some light on them:—

"We are asked to believe that all these 'may be's' happening on an enormous scale, in order that we may believe the final Darwinian 'may be' as to the origin of species. The general form of the argument is as follows:—'All these things may have been, therefore my theory is possible, and, since my theory is a possible one, all those hypotheses which it requires are rendered probable.' There is little direct evidence that any of these 'may be's' actually have been."[2]

And that these strictures apply to more than mere details and trivialities is amply proved by this one passage, which has many fellows, when set alongside of the second passage, which, alas! has many, fellows too.

First Passage.

"In my opinion the greatest error I have committed has been in not allowing sufficient weight to the direct action of environment, *i.e.*, food, climate, &c., independently of natural selection. . . . When I wrote my 'Origin,' and for some years afterwards I could find little good evidence of the direct action of the environment; now there is a large body of evidence and your case of the *Saturnia* is one of the most remarkable I have ever heard."[3]

The case of *Saturnia* was this, as Mr. Beddard tells:—"A number of pupæ were brought in 1870 to Switzerland from Texas of a species widely different from European species. In May, 1871, the moths developed out of the cocoons (which had spent the winter in Switzerland) and resembled entirely the Texan species. Their young were fed on leaves of the *juglans regia* (the Texan form feeding on *juglans nigra*), and they changed into moths so different, not only in colour but also in form, from their parents, that they were reckoned by entomologists as a distinct species."[4]

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[2] "North British Review," 1867, i., p. 313.
[3] "Life and Letters," iii., p. 158.

Mr. Darwin says that when he wrote "The Origin," he could find very little evidence about the direct action of environment. The little surely should have suggested to him the necessity for more, and for waiting to observe for himself; then "The Origin" might have been more what he says it ought to have been. And this is in every respect the more extraordinary that Mr. Darwin had fullest warning from those who had most nearly anticipated him. Hegel laid it down that because of conditions certain results in the way of structure in animals and men were to be found only in temperatate zones and along mountain ranges. Lamarck very directly and definitely ascribed some of the evolutionary changes of structure to changes in the environment, some to the motions of organic beings and some to both of these combined. Buffon laid down direct action of the environment as a cause of evolution. One of his laws was that many species are being perfected or degenerated by the changes in land and sea ceaselessly proceeding, by favours or disfavours of Nature, by food or elements in food, by continuous influences of climate, or changes of climate. Temperature, food, climate were with him the three great factors in the development or degeneracy of animals. And yet Mr. Darwin, in face of all that, could insist and go on for years insisting on practical independence of natural selection to environment.

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[4] "'Animal Colouration," pp. 51, 52.

Second Passage.

"We *clearly see* that the nature of the conditions is of subordinate importance in comparison with the nature of the organism in determining each form of variation—perhaps of not more importance than the nature of the spark by which a mass of combustible matter is ignited has in determining the nature of the flames."[5]

You see how in his letter from which our first passage is drawn he completely takes the availing foundation from this utterance, which, he declared, was the result of his "*clearly seeing*." I call this, by itself, simply unjustifiable, short-sighted, partial, empty theorising and dogmatism. Then Darwin himself told plainly that, later and with more reflection, he certainly would not have stuck to the term "Natural Selection," but would have found another. "Talking of 'natural selection,'" he writes, "if I had to commence *de novo*, I would have used 'natural preservation.'"[6]

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[5] "Origin of Species," p. 8.

I do not say that into the one phrase might not have been imported as much dangerous stuff as had been imported into the other, and caused Darwin to wish to cast it, like a Jonah, overboard ; but the fact is enough, from my present point of view, that he wrote above what he did ; and thus I cannot help thinking that I am justified in asking what I do now ask—Why should we now be called on to bow down to that phrase, and to what it connotes, as to some fetish and fetishes ?

And some of those who put themselves before us as, in the main, following Darwin in his doctrine of evolution and natural selection, yet, in truth, far more directly follow, here, at all events, his predecessors—Lamarck, Buffon, &c. Such writers (and sometimes even Dr. A. Russel Wallace) we are inclined to pose with the question, when they say such and such a thing is explained easily by Mr. Darwin's theory of natural selection, &c.—Which natural selection, first second, or third, do you mean ? Here is a case :—

"The most cursory consideration of the action of environment shows how profoundly it determines form ; of this no better examples can be found than those furnished by the habits of plants. It is easy to see how submerged leaves must become dissected, or desert plants tend to become succulent ; how ever-greens are only possible in certain conditions of climate, or *thorns are only useful where herbivorous mammals abound.* In the same way we can see that the conditions of life profoundly influence animal form." [7]

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[6] "Life and Letters," ii., p. 346.

Here the cart is, to far too great an extent, put before the horse; a purely secondary and accidental circumstance, as we have elsewhere proved by abundant evidence, is put in place of primary and essential. The experience, the invariable experience, of desert plants is that spines and thorns are the direct results of the action of dry and arid air on them, combined with dry and arid soil. Mr. Grant Allen errs exactly in the same way in his articles, "In Nature's Workshop." It is precisely the same kind of reasoning which Mr. Darwin and his followers apply to rudimentary organs. They lay it down that they are useless, in order to relieve natural selection from an apparent failure. Thus "rudimentary organs," from *being useless*, are not regulated by natural selection. But the assumption is too large and is not completely justified by fact. These so-called rudimentary organs make call on the vital functions in so far as long as they exist; and, if they are in no sense useful, natural selection then fails in not entirely and absolutely abolishing them. Dr. Weismann says:—"The complete disappearance of a rudimentary organ can only take place by the operation of natural selection; this principle would lead to its elimination, inasmuch as the disappearing structure takes the place of the nutriment of other useful and important organs."[8]

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[7] C. C. on Parasitism in "Encyclo. Britannica."

Where doctors differ, how shall we agree!

Now, what is, for one thing, practically demanded in view of all this? Clearly an edition of the "Origin of Species," properly edited, rewritten, annotated, and with the insertion of all such cases (evidence) as Mr. Darwin became aware of later, and could not then render wholly available for full and satisfactory correction of his position in "Origin of Species." Mr. Darwin did, in one of the later editions of "Origin of Species," make some small alterations—one of them due to the observations of Mrs. Blackburn, of the young cuckoo turning out eggs and young of the foster-parents, but he did not by any means go far enough there even, and wholly ignored other points quite as important.

Yes; and while the facts are as I say, and these nobody can dispute, newspapers are filled with self-interested laudations of this confessedly incomplete, erroneous and self-contradictory book. It is issued independently by itself as among the best books, and the critical police are so slow in scent that they throw up hats, exulting and roaring over it as one of the best hundred books, &c., &c. Let them roar! they cannot alter the facts. Mr. Darwin himself had to acknowledge where his book was erroneous, wrong—

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[8] "Essay on Heredity," i., p. 88.

utterly wrong—far out, even, as regards its central principles, and such as, had he but had the chance of beginning it *de novo*, would have been very different; and I for one want to have alongside of it a copy of the book as it would have been begun *de novo* years after.

And so "The Origin of Species" goes on, with all, or nearly all, its original errors on its head—a monument of English consistency and determination to stick to its idols, though the feet of clay and the streaks of clay all through are confessed even by the producers of the idols themselves.

"The Origin of Species" is thus not a suitable book to be so presented *alone* to large classes as though it represented in any way the Master's final thoughts about the most important subjects; it should have been completely rewritten, in view of such use and such a confession as we find above; and the Publishers' Congress might have discussed this point of ethics as well as the evil done by cheap magazines and prints to the interests of true literature. There is, from the ethical point of view, a bond between the two far closer than appears at first sight.

"The Origin of Species," *as it stands*, is a book for scientific men alone to study in search—by careful comparisons of first and last—for the strange processes of Mr. Darwin's mind, and the odd ways in which he advanced to his final positions. It is not, in its existing form, a book to be sold as a popular book, because in no sense does it carry its needful corrections with it, and, therefore, it is a constant cause and source of misleading readers as to Mr. Darwin's own final positions, and what he came to regard as the central facts of nature.

Again, says Mr. Darwin:

"I do believe that Natural Selection will generally act with extreme slowness, only at long intervals of time and only on the few inhabitants of the same region. I can see no limit to the amount of change which may have been effected in the long course of time, through Nature's power of selection—that is, by the survival of the fittest."[9]

Which, as we have proved by instance already, it certainly does not by any means invariably do.

Professor Huxley was compelled to admit that Nature does make considerable jumps in the way of variation now and then, and that these alterations give rise to some of the gaps which appear to exist in the series of known forms.[10]

But Mr. Darwin re-asserts his position yet more strongly.

Nature can never take a great and sudden leap, but *must* advance by short and sure, though slow steps.[11]

Don't never prophesy unless ye know, as the Yankee poet-wit said, and don't tie down nature too tightly on a Procrustean bed of your own, for she is original and elastic.

By the time he came to write the "Descent of

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[9] "Origin of Species," p. 84.

[10] "Lay Sermons," p. 312.

[11] "Origin of Species," p. 166.

Man" Mr. Darwin had awakened to the immense importance of conditions of life, and was just then inclined to find them too operative to present him with too easy explanations. Thus, when he finds both sexes of birds so duskly coloured that it would be rash to assume agency of sexual selection, and no direct evidence can be advanced in proof that these colours serve as protection, then it is just *to attribute the result to direct action of the conditions of life.*[12]

There he was moving "slowly along, slowly along" to his proper destination. But the cases of the ptarmigan and snow bunting, which conserve or get rid of colour pigments exactly in the degree of severity of winter were already well-known and indeed notorious, and these cases should alone have stayed Mr. Darwin's pen in many places in "Origin of Species," and as for Sexual Selection and the secondary marks of it, we shall see later that Mr. Darwin was far out.

Mr. Allan O. Hume, one of the most reliable of Indian Ornithologists, and one who has shown faculty for generalisation and wider thought, in his valuable journal "Stray Feathers," of which there are at least about a dozen volumes, presents suggestions of many laws and principles there—one of the most notable being on "Influence of Rainfall on Distribution of Species." After suggesting no end of striking facts, Mr. Hume sums up his final conclusion thus: "*The average Rainfall is the most potential factor in determining the distribution of species, at all events, where birds are concerned.*"[13] (The italics are the author's.)

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[12] "Descent of Man," p. 491.

One glance, and only one, at the vast world of molluscs, &c. It is well-known that on high mountains and in high latitudes land-snails are frequently hindered by the low temperature from assimilating within a given time as much nourishment as others of the same species living on the plain or in a warmer climate, so that they never attain the same size, though they are capable of reproduction. The smallest advance upwards on mountain slopes for such creatures must thus have a decisive effect in that direction whether we can see it or not; it is the most absolute refutation of poor Darwin's fallacy, which he laboured at till he was tired, that no results of such changes are seen *for generations*, which is much the same as if you were to say that if a slow local train to a station a small distance out is started behind a fast train, it does not move at all, though it gets to its destination in shorter time than the fast train gets to the distant one.

The Rev. T. R. R. Stebbing, in speaking of Mr. Darwin's volumes on Cirripedes, refers to "such errors as all foundation works contain," &c., and I wonder whether these errors in "Origin of Species" are of this and no more.[14]

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[13] Vol. vii., pp. 501-2.

[14] *Knowledge*, for April, 1899, p. 90.

"Domestication" was the clear turning point, and in the "Descent of Man" the *volte face* as full as he could make was accomplished. Darwin by that time had made the wonderful discovery that in regard to the most essential matter of reproduction the organs were most sensitive to and most easily modified by change of conditions. Surely, this is a point that Mr. Darwin did not need to wait till that time to have hint of, with Lamarck and Buffon and Oken and Goethe and Hegel all before him. If the following passages are not a direct counterblast to his earlier teachings, summed up in "Origin of Species," I do not know where to look for a counterblast to anything:—

"A much more probable view is suggested by the analogy of the lower animals. The reproductive system can be shown to be susceptible to an extraordinary degree (though why, we know not) to changed conditions of life; and this susceptibility leads both to beneficial and to evil results. . . . . *Very slight* changes increase the health, vigour and fertility of most or all organic beings, whilst other changes are known to render a large number of animals sterile."[15]

Just notice here the utter and illogical vagueness of the words "very *slight* changes" as opposed to "*other* changes"—the logical antithesis is *not* very slight changes. Is that what Mr. Darwin means? It does not define much or distinguish clearly if he does mean it. Again, take this, and read it in connection with passages quoted from "The Origin":—

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[15] "Descent of Man," p. 189, ed. 1888.

"Seeing how general is this law of the susceptibility of the reproductive system *to changed conditions of life*, and that it holds good with our nearest allies, the Quadramana, I can hardly doubt that it applies to man in his primæval state. Hence if savages of any race are induced suddenly to change their habits of life they become more or less sterile, and their young offspring suffer in health (for instance, European dogs in India do); yet they are rarely rendered sterile, though a few such instances have been recorded."[16]

Do the words "very slight" and "other changes" here have the same effect as he means to give them in the former passage, or do they not? If that is clear thinking it certainly is not clear writing, but something like "confusion worse confounded." The sentence beginning "Hence if savages" should have, for clearness, been qualified as to the extent of change, because very slight changes increase the health, vigour and fertility of most organic beings.

"Diminution of fertility (among human beings) may be explained in some cases by the profligacy of the women, but Mr. Fenton has shown that this explanation by no means suffices with the New Zealanders, nor does it with the Tasmanians."[17]

A whole chapter might here, indeed, be written about the possibilities of such negroes as those of the

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[16] "Descent of Man," p. 190.

[17] *Ibid.*, p. 188.

Soudan being in any sense educated, because conditions there are in favour of very early development of the sexual organs. Does Mr. Benjamin Kidd in his "Control of the Tropics" give sufficient scope to this influence?—the early and premature development of the sexual organs—a fact which does much to render impossible the true settlement of Europeans in such latitudes, and at the same time in large measure limits the opportunities of advance even in the native races, so far as these depend upon any long continued culture which requires continued concentration of mind.

The more Mr. Darwin studied the more thoroughly was he convinced of this, and he found it confirmed by plants as well as by birds and mammals. Here is a point:—

"There is hardly anything more wonderful in Nature than the sensitiveness of the sexual elements to external influences and the delicacy of their affinities. We see this in slight changes in the conditions of life being favourable to the fertility and vigour of the parents, while certain other and not great changes cause them to be quite sterile without any apparent injury to their health."[18]

All in the right direction, but all dead against the central principle of "Origin of Species."

Here are two specific instances out of many:—

Abutilon Darwinii is quite sterile in Brazil, unless fertilized by pollen from a distinct plant, but was made slightly self-fertile by artificial treatment in Mr. Darwin's conservatory or hot-house, whereas they are always self-sterile in their native home.

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[18] "Cross and Self-Fertilization," p. 472.

Speaking of certain of the *Fumariaceæ* (climbing plants), Darwin says :—

"Whilst the plant is rather young, only *the outer* leaves, but when full grown all the leaves have their extremities converted into more or less perfect tendrils. I have examined specimens from one locality alone, viz., Hampshire, and it is not improbable that plants *growing under different conditions* might have their leaves a little more or less changed into true tendrils." [19]

Mr. Darwin laid it down that *nature abhors perpetual self-fertilization*, and, indeed, made these his last emphatic words in his "Cross-fertilization"; yet his experiments above cited proved that this was matter of external conditions—indicating, as Hegel in his "Phænomonolgie" and elsewhere had said that conditions were largely the determining forces as to form, character and habit, and that, therefore, certain developments, both in animals and in men, were possible only in the temperate zones or along mountain ranges. Another of Mr. Darwin's bold maxims, and also one of his oldest, needs more qualification than he gave it :—

"Multiply, Vary, let the Strongest Live and the Weakest Die";

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[19] "Climbing Plants," pp. 121-2.

the last emphatic words of his essay on "Instinct" in Romanes' "Mental Evolution in Animals," p. 384. Thus we are compelled to the conclusion that Mr. Darwin, though a fine and patient observer and classifier, had a most insular mind—with small capacities of real independent thought, yet had little or no capacity for assimilating that of others, and, indeed, wrapped up in his own pursuits and conceptions, failed to read and to study where he ought to have done so. We say ought, because had he done so what he admitted to be the greatest error he had committed would not have been possible—had he studied Lamarck and Oken and Hegel as he should have done, so as to have saved ordinary readers from sheer confusion, and us from the necessity of following him up and exposing his errors, contradictions, confusions and ineptitudes.

Even with regard to variations in species at home, Mr. Darwin overpressed his point systematically.

"As with birds, the individuals of the same species inhabiting the same country vary extremely little. I have particularly attended to them; and the rule certainly seems to hold good in this class."[20] All here depends on the exact meaning attached to "extremely little." Is there not a great distinction between domed nests and open nests? yet the magpie, for reasons we cannot get at, will build either in different seasons; or between the deep nests of the reed-warbler hung on reed-stems, and the shallow nests built on strong stemmed bushes and trees. The variations of habit invariably pointing to variations in structure, indeed, are so striking that the interests of field natural history as opposed to systematic book-study lies exactly here. I have seen yellowhammers with much less of yellow than on others and different shades of yellow; cock-bullfinches with faint red, almost fawn, and bullfinches with bright red breasts; cock-linnets in the same locality with red breasts and red-tint on the back, and cock-linnets with no red at all about them, and yet both were breeding. Mr. Chambers writes:—

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[20] "Origin of Species," p. 120.

"A correspondent states that he has seen a variety of the gold-finch marked by strong, distinguishing characters—considerably larger size, more graceful form and much richer and more lustrous plumage—which bird-catchers say occurs frequently in a progeny of the ordinary bird. The distinctions of this animal are greater than those held in many instances to be specific; there seems no room to doubt in such an instance that pairs so peculiar might, in fresh ground of their own, give rise to a race which naturalists would call a separate species." [21]

The palæontological argument is here dead against

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[21] "Vestiges," 12th edition, p. 22-7 (note).

Mr. Darwin. Even those who would fain support him cannot do so here and oppose a general negative. Why, for instance, has a certain reptilian hollow bones like a bird? Were they useful to it or were they not? A *definite* answer to that question either way will be awkward. Mr. Fairfield Osborn's verified facts and his arguments from the development of mammalian teeth do not make that way as Professor Cope and many others have been compelled to confess.

After saying that the complete history of the addition of a number of entirely new elements, the rise of useful structures from their minute, embryonic, apparently useless condition, touches the most vulnerable point in the pure selection theory, this is how he winds up a long and very close discussion :—

" Granting the possibility that the struggle for existence is so intense that a minute new cusp will be selected if it happens to arrive at the right point, where are the now selected new elements, the experimental failures of nature? We do not find them. Palæontology has indeed nothing to say upon individual selection, but chapters upon unsuccessful species and genera. Here is a practical confirmation of many of the most forcible theoretic objections which have been urged against the selection theory."[22]

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[22] "*Nature*," vol. xli., pp. 227-228.

II.

Scientific research more and more goes to emphasize the importance of the very elements which Darwin missed for years, rejected for years, got converted to very late, and never really rose to the immense importance of them as foremost elements in the most remarkable changes. There is, first, the case of the salmon, whose advent into fresh water is directly associated with the withdrawal of blood from the stomach, &c., to the sexual organs, so that while it is using up the extra store of flesh, &c., it has drawn from the sea, it is engaged in the most essential part in the propagation of its own kind. The change from salt water to fresh is signalised by the most extraordinary change that, perhaps, takes place in any creature. All this is brought out by a report, a few years ago, of a committee of the Edinburgh College of Physicians to the Scotch Fishery Board. It is there shown, almost beyond doubt, that the return of the normally-distributed circulation, releasing the sexual organs from excitement and restoring tone and full digestive capacity to the whole of the epithelial cells on the digestive tract ; so that once again it can take full supply of food, and this process is not due to sea-water, because the kelts throw off the discoloured scales and get new scales and renewed brilliancy *after having done spawning and before returning to the sea.* Anglers have raised objections to this, for they say salmon rise to certain lures, and nothing but expectation of eating could lead them to do so. But everyone knows how very capricious "rising" is on the part of salmon then. They may rise and swallow, and yet the digestive process be thoroughly disarranged through the flow of blood to the parts of reproduction and withdrawal from the stomach, &c. Human *gourmets* will rise to titbits when the digestive organs are much out of order. Everybody knows that they will bolt what they cannot digest!

In fact, we should be inclined to contend that all the remarkable changes in certain creatures, from gills to lungs and again from lungs to modified gills or skin breathing, has to do essentially with reproduction and with subtle modification of the organs connected with it.

We find that many creatures, to deposit their eggs, must visit water, often sea-water; and for this purpose need, for the time being, to develop something like, or corresponding to, gills, in one form or another. This proves that they are descended from water-living forbears, and thus their young, at certain stages, are provided with gills, and at certain times they yet develop something corresponding to these—frogs, turtles among them.

The now nearly extinct lung-fish of Australia, that used to be common in the Bennet and Mary Rivers, I do not know enough about to be certain, but I should be inclined to think that the development of lungs in them had to do with necessities that arose upon them, through changes modifying the most sensitive of all their organs.

The slighest influence affecting the sexual parts speedily affects the whole body, more especially those portions of the body which set forth the signs of sexual desire, appetite, or strength. Hæckel says :—

" Every change in the sexual organs reacts on the rest of the body ; so, on the other hand, every important change in another part of the body must necessarily more or less react on the sexual organs. This reaction will, however, generally only become perceptible in the formation of the offspring, which arise out of the *changed* generative parts." [23]

Hæckel also speaks of certain influences which act upon the male organs of propagation only, and affect the structure of the male descendants ; and in like manner, other influences, which act upon the female organs of propagation only, and manifest their effects only in the change of structure of the female descendants. This remarkable phenomenon is still very obscure, and has not, as yet, been properly investigated.[24]

The cases of the cuckoos are good instances in point here. Both males and females have undergone the most marked and decisive modification in several respects—all due to changed habits, as to food, mating, non-nesting, &c.

Yung showed that the relative numbers of males and females among frogs depended on the food which the parents ate. The proportion of females to males increases when a flesh diet takes the place of vegetable diet, and when a cannibal diet is adopted no fewer than ninety per cent. are females. Scarcity of certain foods, and the enforced adoption of other foods, may thus have a very marked effect on whole classes of creatures as to proportion of the sexes, modifying their whole habit, sexual relationship, and actual structure. Polygamy and polyandry are thus, no doubt, induced with all that follows, from the absence or the prevalence of these.

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[23] " History of Creation," i., p. 247.

[24] *Ibid.*, i., p. 330.

Mr. H. Stevenson, in his "Birds of Norfolk" (i., 368), paid particular attention to the Bohemian pheasant, which is so exceedingly light in colour that by artificial selection an almost white variety could be obtained, were it worth while to aim at it. Mr. Stevenson could not make up his mind whether the lightness of colour was due to the soil or to the food; but he had no doubt that it was due to one or other, or to both combined. This pheasant has a particular liking for certain localities.

III.

Weismann, at the start, fell into precisely the same error as Mr. Darwin, no doubt, in some respects, too deeply influenced by the "Origin of Species;" his attempt being, in a strictly systematic biological sense, to fortify pure Darwinism. He divided the cells into two classes—the somatic or body cells, and the reproductive or germ-cells, or germ-plasm; and to effect what he thought the allroundedness of his theory, and give further a logical support to Darwinism, as he then viewed it, the germ-cells, germ-plasm or cells of reproduction were, with him, mere organs or channels of transmission of qualities, common or characteristic to the species, from parent to offspring; the purely individual acquirement not having any part in this or passing over in any way; and he denied that the germ-cells were acted on in any way by influences external or conditions—that these remained mere mediums for transmission of true species qualities. Weismann, as he went on, found, as Mr. Darwin did, that it would not work; and he had to admit that conditions had and have a vast deal to do with it. That action of conditions on these germ-cells or plasmic units was undoubted, so that here variation arose from this very fact, and that through their operation individual requirements might be and were transmitted.

According to this first announced principle of Weismann, and in opposition to Darwin's later ideas, self-fertilization should have prevailed, as conditions at once emerge in the very act of copulation in cross-fertilization in all phases of it. The outward elements are operative in it and cannot be excluded. And even in some cases of what seem spontaneous growth, more especially growth of plants from leaves, Weismann's doctrine will not hold in all. Thus, when the Rev. G. Henslow tells Mr. Romanes that any plant will reproduce itself by its leaves, provided only that the leaves be embryonic (*i.e.*, the leaf not too near its complete development) and that it be not too thin, so as to provide enough nourishment for the bud to form till it has roots, we have a direct testimony against this phase of Weismannism. Further, when the fragment of a leaf of begonia is laid upon moist soil there it strikes root, and grows a new begonia plant capable of sexual reproduction. Mr. Darwin supposed the explanation of this to be that what he called formative material occurs in all cells of the leaf, while Weismann supposes that what he calls germ-plasm occurs in all. But, if so, then where is the distinction between somatic cells and germ-cells? they are inextricably mixed up.[25] If, as Weismann at first maintained, the germ-plasm is insulated and only continuous in and by itself, then, why here is it dispersed, integrating at least all the other cells?

But Weismann did not proceed to this move at once. He made it by many steps, of which, as it is said, certain birds do in snow, he tried to efface the traces by many confused movements round and round.

Mr. Romanes points out that on the appearance of the essay on the "Significance of Sexual Reproduction in the theory of Natural selection," he had some difficulties in following Weismann, and that these were increased when in *Nature* (vol. xli., p. 322), Dr.

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[25] Romanes "Weismannism," p. 64.

Weismann wrote: "I am still of opinion that the origin of sexual reproduction depends on the advantage which it affords to the operation of natural selection. . . . Sexual reproduction has arisen by and for natural selection as the sole means by which individual variations can be united and combined in every possible proportion." And on this Mr. Romanes remarks: "How such contradictory statements can be reconciled I do not perceive, but they furnish a good example of the extreme laxity with which the term 'natural selection' is used by ultra-Darwinians."[26] Quite so; that is what we have said and say: sexual selection in any ordinary meaning of it could never have operated on Prof. Henslow's leaves, or the begonia leaves, that will, on being put on a moist surface, develop a begonia plant.

Precisely like Mr. Darwin with independence of conditions, Weismann, too, had to withdraw his doctrine of polar bodies. "My previous interpretation of the first polar body as the removal of ovogenetic nucleo-plasm from the egg must fall to the ground, about this there is no possible doubt."[27]

And on this Mr. Romanes says: "After his changes of view with regard to polar bodies, unicellular organisms, and the significance of sexual reproduction, nothing remains of his original theory of evolution save what he can manage to retain of his original theory of variation as due to amphimixis. But as we have just seen, he has surrendered this latter theory also. Therefore, at first sight, it appears that no part of the former can possibly remain."

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[26] 'Weismannism," p. 4—15

[27] *Ibid.*, p. 125.

Here is the verdict of another good authority:—

"In the phenomena of unisexual generation we see that the larger organisms never reproduce themselves in the unisexual way, while the smaller organisms reproduce themselves with the greatest rapidity by this method. Between these two extremes unisexual reproduction decreases while the size increases. In the history of all plants is evident the physiologic truth that while the general growth of the individual proceeds rapidly, the reproductive organs remain imperfectly developed and inactive. On the contrary, the principle of reproduction indicates decrease in the intensity of growth and becomes a cause of cessation."[28]

Professor E. D. Cope's careful and exhaustive studies of the effects of impacts and strains on the feet of mammals are testimony difficult for Weismann to explain, since they also meet his requirements.[29]

According strictly to this first idea of Weismann, you had a body of common or hereditable attainment which was transmissible through the germ-cells, and a body of individual or special attainment which was in no way transmissible through these same cells. Dr. Weismann, like Mr. Darwin, failed—absolutely failed—to realise and to present in any way a true idea of interdependence of these two classes of cells. His reasoning was, indeed, much like that of the followers of Darwin, who went hard on the necessary "fortuitous congenital or individual variations" as a material or basis for natural selection to work on. Neither Dr. Weismann nor the followers of Darwin, then, either saw the necessity or felt the duty logically incumbent on them, to account for and to explain how the common or hereditable body of attainment or the "*fortuitous*, congenital or individual" variations had come to be; because, if, from a certain point of view they had asked and tried to answer this question fairly they would have found, and would have had to admit, as indeed poor Weismann found later, that nature does not work in the way of making boxes with secret drawers absolutely shut off, isolated, insulated from all the rest, but that, with her, invariably is a subtle, secret, and absolute interdependence of one on all and of all on one, as Hæckel, especially as to reproduction and organs connected with it, has found all through.

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[28] Talbot, "Degeneracy," p. 29.

[29] "Factors of Organic Evolution."

And the Weismann concealed change of front simply amounts to this, and no less and no more, that the germ-cells or germ-plasm are not independent of influences acting on the other cells in the most vital processes; just as with Mr. Darwin, who at first held that there was something in the inner nature of living beings that acted independently of all outside it, like Weismann's germ-cells, and finally had to own that the greatest error he had committed was precisely this—laying it down that constitution of the creature was everything and conditions little or nothing. Weismann's somatic cells were dependent on much, the germ-cells were dependent on and related to nothing exterior to themselves strictly ; both the great philosophers have had to own they were wrong, that there they had not been philosophic enough ; both tried to turn round, without boldly and publicly and straightforwardly owning it, which is only too well proved by the fact that some of their closest students and greatest admirers do not see and will not frankly admit any *volte face* at all in either of them ; which is truly wonderful, and highly creditable to their insight, when one of the masters himself, in letters, openly declared and deplored his own earlier errors and shortcomings in this very respect, as though in irony to expose and laugh at his too insistent followers ! And it is probable that when we have Weismann's "Life and Letters" he will do exactly the same.

Thus, from one point of logic, Weismann in his first position made an end of pure Darwinism, and *partly* made an end of mixed Darwinism without rightly knowing or realising it ; indeed, believing all the time that, in one way, he was re-establishing it. For if no individual attainment or experience is transmitted through the germ-plasm there is clearly no room whatever for Natural Selection. The type or species model is all that is possible. Hereditarily, there is no room for what Mr. Darwin, with reference to certain wasps paralysing larvæ for feeding their own young larvæ instead of killing them, called a wasp with a "mental peculiarity." At all events, there is no provision under this for the communication to the species in the most direct and effective way of the results of this wasp's "mental peculiarity." Nor, indeed, is there any scope or play for what Mr. Darwin and his disciples, as we shall soon see, have always dwelt on and regarded as potent material for Natural Selection to work on—the "individual variations"; "congenital and non-significant," or, otherwise—what Dr. Ray Lankester has distinctively named the "congenital individual variations," or, more expressly still, the non-significant congenital individual variations. Under Weismannism, in its first form, as denying to the germ-cells any power of conveying individual attainment or experience, you find the assumed period prior to individual variations, according to pure Darwinism, fixed in the essential first prevalence of species or type dominance without essential change, progress, or development. So far, Weismann, as said already, simply endeavoured, logically and biologically, to establish Darwin's first position by securing in the germ-cell one secret sphere wherein life proceeded wholly independent of all conditions—even of the conditions of the living body in which the germ-cells were a vital part—just as though you were to say that the part was greater than the whole and independent of it.

It would not do with Weismann in the last stage any more than with Darwin. He had to admit that certain influences of purely individual origin were transmissible through the germ-cells, that their substance was not isolated, insulated and unchangeable, and, as he had it at first, continuous; and he just found a little corner or two at which they could be acted on by external conditions, and could communicate the results of the same conditions to new individuals.

Weismann, then, exactly like Mr. Darwin himself, cut clean away from the earlier Darwinism and its tendencies; and to answer the question how far Weismannism is a Darwinism, even on this special line, it is, first of all, necessary to enquire which Darwinism is meant—the earlier, the middle, or the later—or whether it is pure Darwinism or mixed that is meant. It is very remarkable to observe and clearly trace out the analogy of process in Mr. Darwin and in Professor Weismann, and in, at least, one aspect it is nothing less than funny, as we hope one day to demonstrate.[30]

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[30] While this was passing through the press, Mr. Edward Clodd, challenged by Mr. Benjamin Kidd—who said he had been studying Weismann pretty well all his life, and in all languages of Europe, and if so, had, nevertheless, missed the essential points—in *The Daily Chronicle*," as to Weismann's change on a main position, replied thus. And his reply is of value here, for his view of Weismann's position is like my own, and that of Talbot and Romanes; and surely we can hardly all be absolutely wrong. This is the summing up of that matter by a reliable hand:—"In dealing with heredity the position of Weismann and others, that acquired characters cannot be inherited, needs a short examination. In his latest work Weismann has practically abandoned the essential basis of his position by admitting that maternal nutrition may play a part in determining variation. He ('Germ Plasm,' pp. 431-565) says that the origin of a variation is equally independent of selection and amphimixis, and is due to the constant occurrence of slight

According to Weismann's first theory the germ-cell, as said already, was wholly isolated from any effect from the other cells. He soon found this could not be maintained, and went on shifting his positions, till finally there was but a supposititious nucleus of the germ-cell, which contained the germ-plasm and was isolated and continuous—the momentary agent in handing down what was insulated and continuous like itself. But in this way, the germ-cell was split into two ; it was not a simple unit any more. It was thus only in a nucleus or part to form new individuals,

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inequalities of nutrition in the germ plasm. As acquired characters affecting the constitution of the parents are certain to affect the nutrition of the germ plasm, it is therefore obvious, according to Weismann's admission, that acquired characters or their consequences will be inherited. This is an emphatic though concealed abandonment of the central position of Weismann." (Talbot, "Degeneracy," p. 79.) It will be noticed that this comes very near to the theory of reproduction in plants, which, in a later part, we shall have to quote from Sachs.

"The Editor of *The Daily Chronicle*."

"Sir,—In his earlier contributions to the fascinating problem of heredity Dr. Weismann formulated a theory that the cell (which all biologists recognise as the unit of the organism) fell, at the outset, into two groups—the somatic, or body-cells, and the reproductive or germ-cells. These last, he contended, remained insulated as the vehicle of transmission of peculiarities—*i.e.*, passed on unaltered from parent to offspring in unbroken series, with the result that characters acquired by the individual are not transmitted. Obviously, the influence of external surroundings on the germ-cells was denied.

"In his later contributions to the problem Dr. Weismann abandons his theory of the 'continuity of the substances of the germ-cells or germ-plasm.' He admits that variation is due to direct action upon its units, thereby surrendering the 'insulated' position. He admits exceptions to the theory that acquired characters are not inherited. He modifies his theory of the structure of the germ-plasm ; but we are here taken into biological metayhysics of idants, determinants, and biophors, which your readers will not care to penetrate.

"I therefore venture to call this the surrender of a 'main position.'

"Yours, &c.,

"May 26." "EDWARD CLODD."

but in no sense could it be or become a living and truly integral part of them. Thus, not only did Weismann abandon his previous distinction between unicellular and multicellular organisms, but he created an *imperium in imperio* in his vaunted simple cell and made it a divided kingdom, if not a kingdom necessarily in certain possibilities divided against itself.

With Mr. Clodd and Mr. Romanes and others, I most distinctly hold Weismann's surrender was that of a "main position," as was that of Darwin in surrendering his practical non-influence of conditions in variation in the "Origin of Species" in favour of the direct and potent influence of conditions in variation in his "Domestication," "The Descent of Man," &c., &c., and the funny thing is that, while Darwin in these later works and in his "Life and Letters" made it quite clear, some of his blind or half-blind disciples and followers will not have it so, and want me and others to close our eyes to it—close our eyes to what the man himself most emphatically and without reserve admitted and regretted in his later years.

There seems, indeed, to be a provision that just in the degree that ascent in type is secured you come on the presence of some moral quality—you cannot otherwise define it—that demands conditions in which individual recognition and affection shall be exercised. So here we have what is really a second series of conditions subtly modifying the first. Here, indeed, comes in an element not properly recognised by Darwin, which is at the base of all, mysteriously modifying, changing and presenting wholly new and wonderful surprises.

Look, for example, at the way in which Dr. Russel Wallace writes of a certain habit of certain hornbills and their allies, certain toucans, &c. He says:—

"The extraordinary habit of the male hornbill in plastering up the female with her egg, and feeding her during the whole time of incubation, and till the young one is fledged, is common to several of the hornbills, and is one of those strange facts in natural history which are 'stranger than fiction.'"[31]

Mr. W. Cotton Oswell is not an ornithologist—at all events, an expert or exact one; but the following passage does not lose any of its evidential value on that account; nay, it is rather increased by that fact.

> "The ornithological name of the bird I don't know, but it is something between a toucan and a hornbill—neither one nor the other—about the size of a large pigeon, though, if I remember right, more like a woodpecker in build. After marriage the birds select a hole in a tree, and gather a few sticks for a nest; the hen takes some feathers off her breast to line it, and lays her eggs. When this is done and incubation begins, the male bird goes to the nearest pond and brings wet clay, with which he stops up the hole, at which his wife went in, leaving one narrow opening in the centre, and through this the excellent fellow feeds mamma and little ones until the latter are fledged and ready to leave the nest; then he and she, from outside and in, jointly peck away the clay, which has, by this time, under the dry heat, become as hard as a brick, and madame and her family make their *début*. The poor monsieur is a rickle of

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[31] Wallace, "Malay Arch.," p. 137

bones, madame as round as a ball. The Kaffirs, knowing this, always dig her out as a tit-bit whenever they find the nest. And what's it done for? An African wood is filled with all sorts of cats, and without a protection the toucan (that's not right, but let it stand) family would soon be improved off the face of the earth, for a hole in a tree comes handy to a cat, but the clay very soon gets too hard for his claws, and the bird hatches in security. Now come with me towards a Kaffir kraal, such as those of the Ba-Quaina or Ba-Wangketsi, permanent tribes. We walk through the outskirts ; there's our friend the toucan again, but there's his wife too, and they keep alternately flying to and from that hole in the tree, out of which many gaping mouths are protruded at each visit. They are the same birds, but the house door is open. Within a radius of five to six miles of every large kraal no cat exists. The Kaffirs kill everything that runs upon four legs for food or clothing ; the best carosses are made of cat-skins—I have one with thirty-six pussies in it—and the birds have found this out."[32]

The birds have found this out. Yes; and their whole habit of nesting and brooding is altered by it—only, mark, in the conditions, however, that demand it. Here, as Mr. Darwin ought to have seen, even in the days when he wrote the "Origin," conditions most directly and intimately affect habit and resource, and become inseparable from them all in the direction of "survival of the fittest," and modification of physical structure and function—the specific development of new resources for the safety of the young against definite enemies carrying with it something so closely correspondent to human reason and affectionate devotion that new and tenfold interest arises on the study, due really to the fact that these creatures are, after all, so like to us in their higher moods, their actions and resources for the safety and preservation of their young. One expert found a starfish—very low, as Romanes would say—very keen to follow and find its eggs. Apart from this consideration, the phenomenon would be only curious, strange and in a way wonderful; with this added quality it is all explained and made natural even, so to say, but only by the admission of the evidence of moral quality superseding pure original instinct, and directing reason, without which it would remain a mere problem for the scientifically curious,—"extraordinary," as Mr. Russel Wallace says, "one of those strange facts that are stranger than fiction." To bring this under a simple scientific law is not to rob it of its remarkable significance or strangeness, but only to recognise the fact that, along with the development of definite affections due in so far to limitation of progeny and definite individual care for them, you have introduced a new and striking element modifying and most subtly elevating all the forces that formerly were recognised as working for natural selection. And this is not in favour of Mr. Darwin's dictum that the nature of the conditions is of subordinate importance, for here the point of origin of these extraordinary and strange habits lies actually in the conditions working on elements some of which Mr. Darwin hardly recognised.

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[32] "Big Game Shooting," i., pp. 134-5.

Dr. Bowdler Sharpe, and I think rightly, on account of two habits, connects the hoopoes with the hornbill family. One is the habit of throwing the food up in the air and re-seizing it; the other the close attendance of the male hoopoe on the female in the nest. The male does not plaster the female in the tree as the hornbill does; but the male hoopoe actually brings all the food, and, though the female comes out for a fly, she does nothing more. Were the hoopoe much exposed to cats, I expect the male, like the hornbill male, would plaster in the hen in the nest, or resort to something similar to that.

But the disciples or co-workers fairly, and in face of all his later warnings, outrun the Master. See how Dr. Russel Wallace gets over the difficulty by what are really assumptions and nothing else:

"Whatever is *really* the fittest can never be destroyed by Natural Selection, which is but another name for Survival of the Fittest."[33]

And I say that by such reasoning, assuming cunningly the very thing to be proved, you could easily prove anything; but the two masters here contradict each other—flatly contradict each other, and we cannot hold by both of them. For just think a moment—the above amounts to this, that what is destroyed is proved by this fact absolutely not to have been *really* the fittest, whatever appearances may say on the other side, and whatever facts may be presented. Whatever case we bring to the notice of Dr. Russel Wallace, all he has to reply is: "Well, now, really, you know it could not have been the fittest, because what is really the fittest cannot be destroyed," dogmatism of the worst kind veiled under terms of reasoning; and, in all apparent simplicity, Dr. Russel Wallace launches us into the depth of metaphysics about *really* fittest. For, as Professor Bain well showed of old, everything in science and logic goes by correlatives, you know, and we have nicely to distinguish, at Dr. R. Wallace's hest, between the REALLY fittest and the *apparently* or *unreally* fittest, and that abstract discussion quite profitless might be endless, and we decline it; only adding that to qualify a concrete phrase, fittest, with what is in reality at basis a purely metaphysical phrase involving a question of reality and non-reality, is an old device which should not be followed. But here Dr. Wallace simply annihilates his own proposition in the attempt to save it. He empties absolutely the one phrase to press in the other, and his precious sentence, logically analysed, simply means *that which survives survives*; "reality" and "fitness" are both put out of court as "minors" not admissible to yield evidence; and Dr. Russel Wallace, while trying to recover for us something of law and ethics, without seeing it banishes both under chance, like the others. Instead, then, of greater definition you have less; your proposition itself explodes, leaving you with no basis other than this, that all which lives lives, and all which dies dies, and "*there's an end on't*"—a pretty philosophy, after Shakespeare's Gravedigger. Surely, surely, a masterpiece of science and philo sophic thinking, and of clear distinction and definition. No; in his case, as in that of Darwin as regards instinct, instead of defining it in any clear and careful way, he resorts to the use of adjectival forms in front of it often not very enlightening, and we protest seriously against the easy, easy process of sticking a term like "REALLY" in front of "fittest," and thinking that this really leads to a solution of the problem, whereas it only throws a mist about it which may confuse, but not render clear. Fittest is everywhere *really* fittest—there can be no two opinions or contrasted kinds of fittest, nor degrees of it, properly, and here, according to Dr. Russel Wallace, there is indeed no fittest at all, that is, none that can be tested in any way—fittest is transformed to suit him into an absolute translation of "survives," nothing more, nothing less, and instead of advance we are really back again on the old track, which led the deep and philosophical poet Pope to say, "Whatever is best, is fittest"—nothing more, nothing more.

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[33] Darwinism, p. 425.

And the most extraordinary thing of all is that, though the "Origin of Species" followed the "Voyage of the Beagle" years after, cases cited in the "Voyage of the Beagle" and in yet earlier writings, most distinctly attest Mr. Darwin's later ideas, and not those of the "Origin of Species," which shows how limited were his powers of thinking compared with his powers of observing, scientifically arranging and classifying.[34]

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[31] Mr. Hutton, in his recent book, makes a point of this, to prove that Darwin anticipated, at an early period, all his latest theory! "It is *not* true that second thoughts are best," &c., &c.

Why, are not his remarks even about the complete confidence of innocent creatures towards men who first visit hitherto unvisited or rarely visited islands—their perfect fearlessness and so on, the most direct proof of conditions as bearing on the most intimate character of these creatures. He and his friends, by killing for specimens on the Galapagos islands changed the conditions for these creatures, aye, and changed their whole instinct and bearing towards man! Hundreds of instances could be given where Mr. Darwin "seeing, saw not," else already the central idea of "Origin of Species" would have been impossible to him.

IV.

Then, while we find Mr. Darwin professing the fullest regard for law, an utter hatred of lawlessness, a profound regard for cause and effect, for order and ordered harmony, in his writings, when he is not consciously thinking of this, he is constantly slipping into phrases which, if they have any meaning, mean, and can only mean, on any natural interpretation of them, the very opposite of what he has said about law and order. Looking through his books once again, I say I can't, anyway, get rid of "chance" with Mr. Darwin, in spite of all his pretended regard for "law" and dislike of "lawlessness" in Nature. Am I not justified in this statement?

"A vast number of eggs or seeds are annually devoured. . . . Yet many of these eggs or seeds would, *perhaps*, *if not destroyed*, have yielded individuals better adapted to their conditions of life than any of those who *happened* to survive.[35]

Happened, and yet law and selective order obtain, and Dr. Russel Wallace finds all the facts as we see them magnify "Survival of the Fittest." Why should men walk together unless they are agreed? Besides, the lower in range, as regards seeds, at all events, there is more, literally, of indestructibility—the higher you go there is the less—so that natural selection has the best chance where it is not needed—it has the least where it is most so. The seeds of weeds and wild plants were eaten by birds, but in most cases not destroyed; nay, they were suited and prepared for germination by the heat and moisture of the birds' body; but developed fruit in a vast number of cases causes the complete death of the seed in process of digestion. "Survival of the Fittest" on this line, eh?

"*Happened*," above, should most clearly in all Mr. Darwin's theories have been "selected" or "chosen to survival." Mr. Darwin speaks of "the wasp that first learned to sting the larvæ in the nerve-centres, so paralysing and not killing them, and thus having fresh and not dry meat for its dependents, as first doing so either by mental peculiarity or by *chance*," words utterly inconsistent with all he professes and claims as regards law, order and ordered harmony.

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[35] "Origin of Species," p. 68.

As to the case of the seeds it lies clear and against Darwin strictly in these words of Professor Henslow:

"The principal struggle for life takes place at the seedling stage, before any varietal or specific characters have appeared, and unless there were a large number of the seedlings which would ultimately bear the improved flower, or else a superior constitutional vigour be guaranteed to be correlated with the particular varietal characters to be preserved, these alone could have nothing to do with the survival of the fittest."[36]

Dr. Russel Wallace says:—

"Of the whole number of the increase produced annually, only a small percentage of the best adapted can be preserved."[37]

Mr. Darwin appears to take exactly the same view at one place, though hardly at another. We find him writing:—

"The real importance of a large number of eggs or seeds is to make up for much destruction *at some period of life*; and this period, in the great majority of cases, is an early one. If an animal can in any way protect its own eggs and young, a small number may be produced, and yet the average stock be fully kept up; but if many eggs or young are destroyed, many must be produced, or the species will become extinct."[38]

But what of the development of something correspondent to care of the young, which we find invariably developed as we advance in the scale, and see parental devotion, asserted even to the point of risking death for eggs and offspring. If the assumption is that eggs and seeds destroyed are as vital and have latent power to produce as good products as those that happen to survive, what of survival of the fittest?

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[36] "Origin of Floral Structures," p. 333.

[37] "Darwinism," p. 413. What of sentence at page 44?

[38] "Origin of Species," p. 52.

Mr. G. W. Bulman, with reference to Mr. Darwin's statement about the vast numbers of seedlings of native weeds destroyed chiefly by slugs and insects, out of 357 no fewer than 295 being destroyed, writes thus:—"Is there, then, anything to show that the 295 plants destroyed were less vigorous than the 62 survivors? There is no evidence or *à priori* argument to prove that slugs and insects choose for food the less vigorous seedlings. Putting oneself in their place—the only method available in the absence of experimental knowledge of their tastes—we should say the more vigorous and better-grown would be chosen." [39]

In the immense mass of eggs and caterpillars of moths and butterflies, which fall a prey to the tiny *Pteromali*, *Microgasteri*, and *Tachinæ*, is there reason to suppose that there is any tendency to spare the healthy and robust, and attack only the weak and unavailable.

Yet, Professor Huxley, supporting Mr. Darwin in a letter, can thus eloquently write in self-defence when such inconsistencies are pointed out to him:—

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[39] *National Review*, xviii., pp. 60-1.

"The man of science knows that here as everywhere perfect order is manifested; that there is not a curve on the waves, not a note in the howling chorus, not a rainbow glint on a bubble which is other than a necessary consequence of the ascertained laws of Nature; and that with a sufficient knowledge of the conditions competent physico-mathematical skill could account for, and, indeed, predict, every one of these 'chance' events."[40]

Then, why on earth did Mr. Darwin, this man of science, allow himself licence in the use of such loose phrases as "chance" or "happened," "accident" and "accidental."

Again, read this:—

"It is not a little wonderful that such an accusation as this should be brought against a writer who has over and over again warned his readers that when he uses the word 'spontaneous,' he merely means that he is ignorant of the cause of that which is so termed, and whose whole theory crumbles to pieces if the uniformity and regularity of natural causation for illimitable ages is denied."[41]

Surely, surely, the word "spontaneous" is most illegitimately used with reference to anything of which Mr. Darwin is ignorant as regards cause.

"The mind is the measure of the man"; but, if

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[40] "Life and Letters," ii., pp. 129-201.

[41] "Darwin's Life and Letters," ii., p. 119.

Mr. Darwin is to set down as "spontaneous" whatever he fails to know the cause of, he constitutes his own mind, in its ignorance as in its knowledge, the measure of the universe.

Mr. Grant Allen here as elsewhere follows suit. He speaks of the survival of wasp-queens thus:—

"There are several hundred females in the nest of equal rank, and the question which or how many among them are finally to become the foundresses of new nests is decided in *Nature's rough and ready fashion*—by the *chances* of survival."[42]

But it is survival of the *fittest*, mark! And "chances" there really reduce it to those that survive. What have Mr. Darwin and Mr. Grant Allen to do with *chances?* Their theory is all to be reconciled with law, with order, with uniformity, in which there can be no *chance* nor *happening!* It is selection, order of survival, as of all else; and I, for one, seriously object to and protest against the use of such loose, most indeterminate and misleading phraseology in matters of science.

We find Mr. Romanes saying deliberately:—

"The theory of Natural Selection, as such, furnishes no warrant for supposing that the same beneficial variety should arise in a number of individuals spontaneously. On the contrary, the theory of Natural Selection trusts to the *chapter of accidents* in the matter of variation."

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[42] *Longman's Magazine*, vol. xxxii., p. 522.

Now, here what does *spontaneously* mean? What does "*chapter of accidents*" mean? Are we to take the words in their ordinary natural sense, or are we not? Are we here in science to be involved in worse results than have followed the non-natural interpretations of theology? If so, Mr. Romanes admitted *chance* in his scheme; if not, what does he mean by these two phrases slipped in in this way? And I implore, for clearness and satisfaction and the interests of science, that his friends and disciples should plainly answer me. "Chance," says Mr. Darwin, "is the combination of several systems of causes, which are developed each in its own series independently of the others." Here, as in so many other cases with Mr. Darwin, we have really a series of contradictory assumptions instead of a definition, and Nature reduced to man's mechanics.

(1) How can you have systems or combinations of causes, or of anything else, if each in its own series is developed independently of the others? "Developed each in its own series"—that fetches me! If a series, then, indeed, causes must produce causes absolutely in series; or are we to figure this series as causes mixed up with effects in a kind of unrecognised and accidental alternation? Or does Mr. Darwin mean that from this series of causes no effects are to spring till his wonderful mechanical combination comes, which is *chance?* Here is a bit of loose world metaphysic that wants more wit than mine to make it clear to my own mind, not to speak of others' minds!

(2) It is, however, most absolutely clear that the development of each series independently ceases the moment that there is perceived in them a clear tendency to combine (and in the combination become something else); indeed, the potentiality of combination in them is the great fact. Thus, they are not from the first *independent* of each other. Scientifically, as Mr. Darwin says, their final end and the inevitable necessity of their process, as he himself posits it, is this very combination; and the law or the necessity under which they lie to combine is that which, philosophically, makes it impossible to view them wholly apart, this being once seen and admitted. When a tendency of two elements or substances to combine is perceived and demonstrated, you have the verification of a law of affinity—a law absolute and unfailing and not dependent on your seeing it or not seeing it—the combination is a demonstration to human intellect by translation *from* ignorance and the imagined field of chance or possibility into that of assured law. What before might have seemed chance, so long as the tendencies were unfollowed and the results unverified, now becomes a recognised truth of science, the law of it clearly understood and accepted, so that Mr. Darwin in this place comes perilously near to asserting logically that chance is law and law is chance. The combination is the revelation of a law of affinity; and Mr. Darwin here writes pure nonsense when he seriously says that " chance is a combination of several series of causes,"

&c. This sort of thing admitted, there is an end of thinking and you are engaged in a loose game of mere verbal thimblerigging.

Dr. Russel Wallace is very decided in his assertion that we must get rid of the idea that chance determines which shall live and which shall die;[43] but in that case, why did the Master, why do so many of his disciples so favour such loose and misleading words as "chance," "happened," &c.; surely there is nothing sacred to "selection" and "survival of the fittest" and "reign of law" in them?

Mr. Romanes may have been as desirous as Mr. Darwin was to get rid of *chance*, but certainly he was hardly thinker enough to achieve it. Read :—

"The first mode of origin consists in natural selection and survival of the fittest, continuously preserving actions which, although never intelligent, yet *happen* to have been of benefit to the animals which first *chanced* to perform them. Thus, for instance, take the instinct of incubation. It is quite impossible [Might it not, now, have *chanced* or *happened?*] that any animal can ever have kept its eggs warm with the intelligent purpose of hatching out their contents [Just as one might say no bird ever built a nest with the intelligent purpose of laying eggs in it!]; so we can only *suppose* that the incubating instinct began by warm-blooded animals showing that kind of attention to their eggs which we find to be frequently shown by cold-blooded animals."[44]

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[43] "Darwinism," p. 123.

Now, as bearing on this point, I have many times observed a very peculiar thing, which I could not have observed had I not kept birds of many kinds in an aviary, free to come out into a large room and fly about. It is this: that unless facilities are given by material, foundation, &c., for building a nest, the female mated bird, at a certain point, will reject the advances of the male, as much as to say, "All very well for you, you stupid male creature, but I can have no more to do with you, seeing I have no nest to put the eggs in, till I see something like a nest advancing towards completion." Or, else, the female bird, more wonderful still, manages to omit something essential in the contact; for, even though, as the wilful man must have his way, the cock succeeds in so far, there is not one case out of twenty cases like this where eggs, or, at all events, fertile eggs, result; but give a chance of a nest, and eggs come pretty exact to the time that it is ready. There can be no doubt of this in my mind, from conduct of birds repeatedly seen and noted, and clearly in the case of birds like the long-tailed tit, which builds a very elaborate nest, and needs nigh a month to do it, there must be some approach to the same process, so as to ensure that no eggs are lost, and that eggs are ready, uniformly, as I have observed they are, by about the time the nest is finished. From such facts as I have observed, I justify my interpolation in the extract from Mr. Romanes just above, to the effect that birds directly connect the building of a nest with the laying of eggs in it, with brooding and rearing young in it; for which it is so admirably fitted—the quill-end of every little feather in it having been passed again and again laboriously through the bill till perfectly softened. The wren does not do this by what are called the "extra" or "cock" nests. Therefore, by analogy, I declare that, with regard to incubation, it is performed with a distinct consciousness on the part of the sitting bird that her sitting, if the eggs are not addled, will bring out young birds in due time, which, at first wholly featherless and very tender, the very slightest hard point of quill might hurt. Would Mr. Romanes have declared—do his friends and followers even now declare—that the cuckoo drops eggs into other birds' nests merely mechanically, and with no conscious realization that brooding on the part of the victimised bird will hatch them? A plain and explicit answer to this will oblige. Mr. Darwin says the cuckoos had found this process of benefit to the species and to them, mark you; but how of benefit so, unless by realization, more or less consciously, not only of the process, but of the end? And how, pray, could the cuckoo which, in a proportion of cases, is over the sea before its young have been hatched in the victim's nest, have come to the conclusion of benefit except through this consciousness of the end?

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[44] Romanes' article on "Instinct" in "Encyclo. Brit.," quoted by himself in Chap. xii. "Origin and Development of Instincts" in "Mental Evolution of Animals," p. 177. And all I can say is that such a mixture of chance and happening and law and supposing is surely not in its right place in the "Encyclo. Brit.," of all organs of knowledge, science and philosophy. And what of the continuing to do systematically by the species the actions on which individuals only *happened?* Was that a different *happening* and *chancing*, and how?

POSTSCRIPT.—If Mr. Benjamin Kidd, who, according to his own report, for about ten years past has scarce done aught else than read Weismann and all about Weismann, in all the languages of the wide, wide world, does not admit that here there is a decided change of position, *volte face*, about the germ-plasms, then there is nothing more to be said. He illustrates—aptly illustrates—on that side precisely what some of the blind, or half-blind ones, do about Darwin—will not admit that he changed his position radically, and, more than that, quietly confessed it to friends and correspondents, but did not *effectively* alter his earlier books, which still go on selling—as they were, as they were—with all their admitted errors on their head. We need not wonder much at Mr. Benjamin Kidd and Weismannism—for Weismann has tried to work round and to conceal his clear turn-about—when Darwinians stick up for Darwin as correcting nothing, retracting nothing, regretting nothing; though Darwin himself confessed that he needed to correct much, and of the most essential, too, retracted a good deal and regretted yet more. It would have been more to his honour if he had brought his earlier books into line before going on to manufacture more. But, then, you see, the books went on *selling—as they were, as they were.*

PART II.

SOME SPECIAL ERRORS AND INCONSISTENCIES.

I.

Professor Huxley says that—"The variation from their specific type which individuals present 'are the objects of' the selective action of external conditions." This is but a paraphrase, with a difference —the recognition clear of external conditions—on Mr. Darwin's words in "Origin of Species," p. 34.— "These individual differences afford materials for natural selection to act on." Mr. Romanes follows suit, speaking of "*fortuitous* variations for natural selection to seize upon." Dr. E. Ray Lankester, of course, must follow up. He has instances "fully explained by the theory of natural selection *acting on congenital fortuitous* variations."

I cannot understand that. The first question to be asked is—What are "individual differences," in Mr. Darwin's words; "variations from the specific type," in Mr. Huxley's words; "fortuitous variations," in Mr. Romanes' words; "congenital fortuitous variations," in the words of Dr. Lankester? Is there such a thing as "individual differences," or "variation from specific type," or "fortuitous variations," or "congenital fortuitous variations," which is absolutely independent of conditions? That question arises even on the words of Professor Huxley above, for, according to him, " the action of external conditions," in their " selective character," begins only when variations from the specific type arise in individuals. And hence the rational and most necessary query is—Do these " variations from the specific type which individuals present " arise spontaneously, or are they independent wholly of the action of external conditions ; or, in other words, does the " selective action of external conditions " begin only then ? If so, I ask this—External conditions, of necessity, existed before ; had they in their action, then, nothing selective—not even anything efficient—kindred to that which is so definitively attributed to them afterwards ? I should like an answer to this question, and, either way, there will be something to follow. Mr. Darwin, it is too true, did not at first admit conditions as of any real importance, either before or after this point —significant point—in the " Origin of Species " era, and had to confess that the " most grievous error of his life " was precisely that same. Now, if there is, indeed, such a thing as " specific type," which is produced, as Mr. Darwin, in his first period, argued, without reference to conditions, and which, if it exists, must have been produced under one unvarying set of influences, what are, and can only be, the influences which go to produce " individual differences," or " variations from the specific type," or " fortuitous variations," or " congenital fortuitous variations " ? Clearly, in their case, there must have operated something, whether we can detect it or not, which did not operate on those representing specific type. (2) Specific types, did they come into existence wholly without the operation of natural selection, as defined and accepted by these gentlemen? This seems to be implied in these utterances taken by themselves. Now, how did these "individual differences" and "variations from the specific type" come into existence? Not by natural selection, because they, having come, "afford materials for natural selection to act on," presumably not truly present before, or they "are the objects of the selective action of external conditions"; if they had not come, natural selection would not have had materials to work on, mark you; therefore could not have been at all in action before; the words are "materials," not fresh materials; materials absolutely unqualified. It thus follows natural selection began its action only after such differences or variations had appeared.

Here, then, you have a "specific type," which has come to be what it is, on the proposition of these gentlemen, without the existence of natural selection, or the "selective action of external conditions." According to this, such "selective" action only comes into play *after* the appearance of individual differences, or "variations from the specific type." So that here, logically, you have "specific types" attained by one set of influences, and "natural selection," or the selective action of external conditions "initiates another set of influences, which come in to operate on these differences" or "variations"; but I here beg to emphasise the fact that the production of "individual differences," or "variations from specific type," or "fortuitous variations," or "congenital fortuitous variations," come into existence in an absolutely unexplained, and even unrecognised, manner, hanging like a cloud, utterly without support, between the specific type, however produced, and the "individual differences" or variations, which are to form a ground, or "material," for "natural selection," or the selective action of external conditions, to act upon.

No doubt Messrs. Darwin and Huxley were great men, and made valuable contributions to science and literature, but here I say simply that in a case where thought should be able to follow thought and find satisfaction in it as a connected, complete, and consistent whole, so far as it goes, explaining what else were confusing or even contradictory, I find it unusually like a bridge that is set down as having three arches: (1) specific type; (2) individual differences or variations from that the genesis of which is wholly unexplained, undefined, and taken for granted; and (3) "natural selection" or "the selective action of external conditions," basing on and taking rise from these "individual differences" or "variations." The second arch is all in fragments, or rather does not at all exist — it is a gap filled up with mist or cloud, and yielding no proper footing on which to walk from "specific types" to selective action or natural selection working on their "differences" or "variations."

Then, Dr. A. Russel Wallace comes in with "simultaneous variation" as a new element, and where then is the "specific type" so loudly vaunted by Professor Huxley? Dr. Wallace writes:—

"What Mr. Romanes regards as 'a very large assumption' is, I maintain, a very general fact, and at the present time one of the best established facts in Natural History. A brief summary of these facts is given in my 'Island Life' (p. 57), and I possess in manuscript a considerable collection of additional facts, showing that simultaneous variation is a general phenomenon among the best known species of animals and plants. Unfortunately, very few naturalists pay attention to individual variations."

These are Mr. Romanes' words:—

"A very large assumption is made when it is said that the same variation occurs simultaneously in a number of individuals inhabiting the same area."[1]

If Mr. Romanes, like Professor Huxley, went for the "specific type," of course, it is easy to understand his concern not to admit the "simultaneous variation" which Dr. Russel Wallace goes for so strongly, for in the "simultaneous variation" the type specific or other is no longer to be found intact as of old—it is wiped out or on the way to be so—as Artemus, the delicious, says, "it has no existents, it is a myth."

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[1] *Linnean Journal*, xix., "Zoology," p. 343.

A further difficulty—how long may this specific type remain fixed and uniform; by being specific it should be approachably fixed, and, being a type, it should maintain certain important unchanging lines, whereas even here in Mr. Darwin and Professor Huxley you have most distinctly set in action a selective power within a certain space of time to end it, and, instead of the slow, slow uniformitarianism, you really have from them logically a new starting point, with wholly new influences at a definite moment introduced.

(1) "Specific type" presented to the mind, as though it were something at least apparently fixed, and had for a definite period of time been so fixed; then (2) the sudden emergence of new results in "individual differences" or variations from it; then (3) on these the opportunities or "materials" for "natural selection" at last to come in with its "selective action of external conditions." Did ever one hear of such an inconclusive, contradictory medley in so few words? Almost all over it is the same; these profound gentlemen, who have had full introduction to the secrets of Nature, cannot express themselves when they really come to think with the most ordinary coherence, and sometimes talk utter and unmitigated nonsense, not seeing clearly the drift of their own words, or recognising where their statements lead them, in this case really to dead walls, and fresh starts on the other side of them—what really amounts to a new creation—the very last thing they would up-

hold, the very last thing they are aiming at countenancing in any way, and very, very unlike slow, slow dead and alive uniformitarianism, at all events! Either, then, specific type is like Hegel's Absolute, never realised, but only a becoming; or else, on the other hand, you go for pausing points, new starting places, new creations, in a word, the very thing you set out to demonstrate was impossible.

II.

The full title of Mr. Darwin's book was "The Origin of Species by Means of Natural Selection; or, the Preservation of *Favoured* Races in the Struggle for Existence." Now, here the word "favoured" is surely terribly out of place! Mr. Darwin could not there have realised what it means. "Favoured!" how? By any exceptional recognition of some power above the process working, as he says, infallibly, if slowly, slowly, on principles of law and order, successions of causes and effects. "Favour," in any ordinary meaning of the word, can have no place in a scheme so inflexible and so obedient to obtaining laws, to which there are and can be no exceptions. Inflexible! that is it. Mr. Darwin is careful when he is put to the test to declare himself absolutely a devotee of law. Law or Nature can permit no favour; it is inexorable. Yet Mr. Darwin from first to last is constantly using expressions on a par with the above, which were quite enough to justify the accusations raised against him, which he tried, with but a qualified—a very qualified—success, to repel, such words as "fortuitous," "chance," "accidental," "happened," &c.

Mr. Darwin professed to believe that if we could only see how every variation had come to be what it was, only the law of cause and effect would be fully verified for us. Yet he, he of all men, was constantly using phrases directly in the teeth of that—"fortuitious," "chanced," "accidental," "happened," &c.—and as he grew older and saw more and was able to note exceptions that convinced him "conditions" had a vast deal to do with variation which hitherto he had deemed insignificant, quite secondary, and so on, he more and more came to lay it down, as we have seen, as regards seeds, eggs, &c., that whole crowds perished, among which may have been thousands of equal or even of *superior* character to those that survived. Here he may have come to see more clearly the working of cause and effect, but I say that he at once put the touch of decomposition to his own argument, his own creation, and, what is more, wholly emptied his universe of all moral and even equitable quality, not to say of consistency. For if those that might have proved equal or even *superior* to any that survived went down—seeds eaten of cattle and of birds, young plants eaten of slugs and snails, &c., eggs and young birds destroyed as, say, by cuckoos, &c., what room is there for maintaining truly any law of "survival of the fittest," since admission has been made that the *fittest* may have been destroyed and only the *Fitter* survives? With regard to plants eaten by slugs and snails, there is no ground whatever for holding that only the weak and feeble are chosen; on the contrary, there are very strong grounds for believing that the best grown and most succulent are *selected*, at all events by many species of insects and molluscs; hawks and birds of prey do not go, as far as I have observed, for poor and weakly birds; nay, they have been known after catching such to reject them. The otter catches many fish which it never touches, finding that on their exposure to the air they are tainted by disease; and, indeed, the *destructiveness* we are compelled to confess in many wild creatures we may reasonably conclude to have origin here—precisely here—they reject all save what is strong, healthy, vigorous. The cuckoo would run a risk of not succeeding if it chose systematically for foster-parents, the least robust and enduring birds in their various kinds. A whole range of facts we have in favour of Mr. Darwin's *afterthought*, that often the equals if not the superiors of those that survive go down in the struggle; and if that is the case, if even exceptions to the rule may be found in any number, then "survival of the fittest" does not, in the sense so often asserted, prevail through nature. The whole argument here is this: that those animals which prey on living creatures select, in many cases, the very best and fittest to survive; precisely as man himself selects and rejoices in eating the very finest specimens alike of the production of natural and of artificial selection. The eating of larks in England cannot be said to have extended and improved the breed of larks, because the smaller the creature the greater is the desire for largeness and plumpness in it for table purposes; and when Lord Lilford told us, as he seriously did, that not one-tenth of the larks in rural Northamptonshire near his own place were to be seen in his last days that he had seen in his youth, can it be doubted for a moment that large numbers of those larks that were best fitted to survive and to produce fine birds and fine singers had gone? Nine-tenths of the number that once existed over a given area could not reasonably have all been inferior birds and likely only to produce inferior birds. Both man and the lower creatures, so far as they are hunters, are constantly disturbing the laws of survival of the fittest, and, though Dr. Russel Wallace was right and very effective in his argument that no analogy as between species in wild nature and species under artificial treatment would hold, yet here at one point there is an analogy, and a strong one, too. It is only by such trains of thought based on such facts that such a word as "*favoured*" can be justified in the place Mr. Darwin put it characteristically in his title, though it is really at loggerheads with all the rest of it.

Certain flowers that have abundant nectar are chosen by certain birds for their honey. I have observed by watching them and then examining the heads or clusters after the visits of these birds that all the stronger cups had been mutilated and cut off, and that the young and weakly cups, without nectar or a very little of it, were left. Here the strong were taken and the weak were left—systematically left.

Another point. Speaking of mere "individual differences" or "variations from specific type," Mr. Darwin himself says distinctly that monstrosities graduate so insensibly into mere "variations" or "individual differences," that it is impossible to separate them. Now, if it is *impossible* to separate them over the whole field in this way, what about "survival of the fittest"? Are they fittest to survive which have any touch of monstrosity? Does the law of natural selection embrace, or does it cast out, these monstrosities, as a rule, or does it not? Does it, by its own inevitable action, tend to reduce them, so as to make the trait still more unrecognisable, or does it not? Still, admitting that, the strain is there; "monstrosities" graduate so insensibly into mere variations or "individual differences," that it is impossible to separate them. This holds not only of natural, but of artificial, selection.

But when Mr. Darwin has based—absolutely based—natural selection on "individual differences" and "variations," and when these graduate so insensibly into such monstrosities, where do you then draw the line about the "fittest?" That question I should like so answered that it would entirely relieve Mr. Darwin from the onus that seems to lie in such terms as this "favoured," in his title of "Natural Selection," and such terms used elsewhere as "fortuitous," "chanced," "accidental," "happened," &c.

III.

Even after Mr. Darwin had got hold of the facts which compelled him to recognise the variation caused by mere change of locality or condition, he tried in vain to make it appear as though the influence was longer in operating than it really is, to escape, as one may say, the full effect of his conversion. He writes:—"We have good grounds for believing that the influence of changed conditions accumulates, or that it is cumulative, *so that no effect is produced on a species until it has been exposed during several generations*[2] *to changed conditions.* . . It is certainly remarkable that changed conditions should at first produce, so far as we can see, absolutely no effect, but that they should subsequently cause the character of all species to vary."[3]

Professor Bailey tells of squashes, and other plants, on which a change of soil, *in one season,* operated so that they might readily have been taken for distinct varieties. The pupæ of the Texan *Saturnia*, taken to Switzerland and fed on *Juglans regia* instead of *Juglans nigra*, at once changed into moths quite different. The effect on the horse in West Africa, as told by Winwood Reade, is AT ONCE evident, and it rapidly deteriorates. The sheep speedily change in other respects than their wool. And not only this, but in one year they lose their points in near France. In dogs taken to Africa the changes, too, are visible in a very short time; many plants completely change their nature on being planted on African soil.[4] Metzger's experiments on various kinds of maize from America, and cultivated in Germany, were definite enough; they showed slight change, especially on the higher seeds, the very first year; in the second they got discoloured and approached to common European maize.[5] In the third generation all resemblance to the American parent form was lost; they were actually ordinary European maizes. We cannot afford much more space to multiply examples, which are numerous, and, under more careful observation and experiment, are year by year so surely increasing.

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[2] Mark, it is generations here—not years—a much longer period.

[3] "Domestication," ii., pp. 261-3.

Darwin himself refers to and, in summary, cites Metzger's experiments on the maizes, and their results, as "the most remarkable instances known to him of the direct and prompt influence of climate on a plant." But there was a whole *catena* of cases of the same kind, and that Darwin could cite these cases, and go on maintaining, in his text, the statement that "we have good grounds for believing that the influence of changed conditions accumulates, so that *no effect* is produced in a species until it has been exposed, *during several generations*, to continued cultivation or domestication," is very noticeable and characteristic. The bald absurdity of the "no effect," in above sentence, seems to have struck a discerning "reader" of the British Museum copy; for he has written, in a very neat hand, on the margin :—"He should have said 'no *appreciable* effect.'" That would have been logically better, as no man can deny, since he assumes and admits there that causes are at work; but even that is not enough, in face of Metzger's facts, and many, many others, that should then have been fully known to him.

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[4] None of the English breeds of sheep can be kept pure in France; the lambs of even the first generation lose vigour as the heat of the summer advances, and the breed becomes absolutely degenerate. It is extremely curious that the force of inheritance, which seems all-powerful in England, should give way so utterly in such a slight change of circumstances.—"Encyclo. Brit.": "Breeds.'

[5] "Domestication," i., p. 341.

Then we must make room for a few more of the most striking typical cases :—

M. Carrière raised the radish of cultivation, *Raphanus sativus L.*, from the wild species, *R. raphanistum L.*, and, moreover, found that the turnip-rooted form resulted from growing it in a heavy soil, and the long-rooted one in a light soil.[6]

The truth is, conditions in all these matters are determinative of shape and form, as well as of some other things. From the same seed I got a close approach to both turnip radish and long radishes simply by sowing some in ground that had not been dug for years, and was consequently hard, and only scratched and softened with a rake an inch or two at the top. I got also in this way from the same seed turnip-shaped beetroot and the ordinary form, the former sown in ground treated the same as that yielding the turnip radishes ; and also I got Sutton's King Carrot approaching to turnip shape in the same way.

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[6] "Factors in Evolution," Cope, p. 227.

Vilmorin converted the carrot, *Daucus carota L.*, an annual, into a hereditary biennial by sowing the seed late in the season, till the character of flowering in the second season became fixed.[7]

Cabbages very rarely produce heads in hot climates, and even when they do they are poor.

Bees taken to Australia cease to store honey after a few years' experience of the mild winters.

Certain kinds of trout on being transported to New Zealand in a few years undergo remarkable changes —cases arising where the marks of transition to traits of salmon are clearly recognised.

Certain seeds of plants taken from New Zealand to Australia and *vice versa* will not grow, or die out in a few years at furthest.

As there is little cold weather in Mexico, and therefore no necessity exists for bees to lay up winter stores of honey, the bees there take it easy, and do not lay up honey stores ; and bees taken there from colder climates in a year or two drop it also, and become exactly like the Mexican bees.

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[7] Cope, p. 228.

Professor Poulton has produced remarkable colour changes in the larvæ of Lepidoptera by confining them to the branches of plants of distinct colours. Thus, Geometrid larvæ confined to the stems of a black colour become correspondingly dark, while those restricted to white twigs become very pale. These larvæ, and still more strikingly those of *Cossus ligniperda*, when confined on branches which supported lichens, became of variegated colours, corresponding with those of the lichens, and affording an admirable means of concealment.

Deer have lost their horns, or their horns grew stunted, on a short voyage, though they recovered them afterwards; and as horns in the deer are absolutely connected in every phase with the sexual organs, here was evidence of a modification in their most sensitive parts effected in a few days by change of condition. Mr. Darwin himself actually tells of a Wapiti deer whose antlers became quite stunted during a voyage, but grew all right again when living under normal conditions. Had the deer been kept in these conditions the horns would only have become more and more stunted.

Professor Cope says: "I have changed the spiny *Ononis spinosa L.*, Rest-harrow, both by cuttings and by seed into a spineless form undistinguishable from the species *O. repens L.* in two years; but it would have, I doubt not, at once reverted to the *O. spinosa* if I had replanted it on the poor soil from which I took it."[8]

Mr. Beddard in his valuable "Animal Colouration," says:—

"Mr. W. H. Smith has remarked that the prolonged drought recently experienced in New Zealand produced pale-coloured varieties; and that the lepidoptera of that country are generally paler on the plains than on the hills. 'The higher we ascend the Alps,' he remarks, 'the more humidity we meet with, and the greater the darkening of the lepidoptera, until we reach the summit, when they become perfectly black.'"[9]

In 1871 the spring flood broke down the barriers separating the two different lakes of the salt works near Odessa, diluting the water in the lower portion to 8° Beaumé, and also introducing into it a large number of the brine shrimp, *Artemia salina.* After the restoration of the embankment, the water rapidly increased in density, until in September, 1874, it reached 25° of Beaumé's scale, and begun to deposit salt. With this increase in density a gradual change was noticed in the characters of the *Artemiæ*, until late in the summer of 1874 forms were produced which had all the characters of a supposed distinct species, *A. muelhausenii.* The reverse experiment was then tried. A small quantity of the water was then gradually diluted by M. Vladiminr Schma-

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[8] "Factors in Evolution," p. 227.
[9] P. 61.

kewitsch, who conducted the experiments, and, though continued for only a few weeks, a change in the direction of *A. salina* was very apparent. Led by these experiments, he tried three others. Taking *A. salina*, which lives in brine of moderate strength; he gradually diluted the water and obtained as a result a form which is known as *Branchinecta schaefferii*, the last segment of the abdomen having become divided into two. Nor is this change produced by artificial means alone. The salt pools near Odessa, after a number of years of continual washing, became converted into fresh water pools, and with the gradual change in character *A. salina* produced a species known as *B. spinosa*, and at a still lower density *Branchinecta ferox*, another species described as *Branchinecta media*. Here not only new species were produced, but a new *genus*.[10]

The transformation of the Mexican axolotl into an amblystoma by dryness of the surroundings is another case. "Here," says Mr. Mattieu Williams,[11] "is a case of very rapid modification in which the animal without the aid of hereditary[12] variation, without any destruction of the unfit and survival of the fittest, has at once adapted itself to varying external conditions—a case apparently supporting the old fashion development theories of the predecessors of Darwin." Again, the axolotl reproduces in warm Mexico as a branchiferous amphibian, in colder climates its fertility is diminished; it becomes a salamandroid before reproducing, thus lengthening life by delaying genesis.[13] I am fully aware that most ingenious efforts have been made to make this out a case of reversion; but, since the transformation either way is speedily effected by change of conditions, this will not work; since, of course, it cannot strictly be a reversion in both cases, that is, to an earlier and to a later condition at the same moment practically. The extreme Darwinites in a word cannot have it both ways, though "they pays their money and they takes their choice."

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[10] Cope "Factors of Organic Evolution," p. 230.

[11] *Gentleman's Mag.*, cclxii., p. 100.

[12] How far is "hereditary variation" identical with the "congenital fortuitous variation," of which Professor Ray Lankester makes so much, as we shall see by and by? how far do they cover the same ground, or overlap each other? I should be very glad to know.

Low temperature dwarfs the common pond snail *Lymnaæ stagnalis*, and not only that, but an increase in the volume of water has a direct effect on the formation and size of the shell, as Prof. Semper and Dr. H. de Varigny have both observed and recorded.

In fact, the whole procedure of a gardener or floriculturist in what is called "breaking up the constitution" of a plant in order to obtain variations, which may or may not finally end in mere "sports," is based on the most striking changes of conditions—sometimes in wet, sometimes in dry, sometimes in strong light, sometimes in shade, &c., &c.—and yet Mr. Darwin went on for years and years scientifically denying it; and his book the "Origin of Species" in so far as it denies the direct influence of conditions operative in natural selection is false and wrong; for I say that Nature in a lesser and less direct way is constantly by change of condition effecting what the gardener does intelligently and directly, and if Nature did not, in her way, practise the same art, he could not practise his art, for he can only go with, not absolutely against, her.

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[13] Ray Lankester, "Longevity," p. 85.

It is to me often richly funny—nothing less than that—to see writers who fancy they are unqualifiedly supporting Darwin only supplying all along the line that which goes to convict his first position in "Origin of Species," of being untenable and absurd, and lays siege, though as it were by a force somewhat at a distance, to his second and modified position, which admitted the force of environment, of conditions—food, light and heat, &c.—but would not admit any evidence of this for generations. Thus Professor van Beneden, in the introduction to his excellent book, "Animal Parasitism," in the "International Scientific Series":—

"Naturalists have not always studied with sufficient care the correspondence which exists between the animal and its food, although it furnishes the student with information of the most valuable kind. These primary relations between living beings and their alimentary regimen call forth the activity of every species."[14]

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[14] P. 15.

And again, Professor Semper, in his "Animal Life," in the same serviceable and handy series :—

"Evidence has been given in this chapter that changes in the degree of saltness in the water exert not merely a selective influence on the animals exposed to them, but also sometimes effect a remarkable modification of them."[15]

The whole section in this work, entitled "Influence of Inanimate Surroundings" (p. 40), in which he shows not only the indirect but the direct modifying influence of food, and alike as regards quality and quantity, with many illustrations of the remarkable changes or transformations effected in the stomachs of animals by the sudden passing from grain diet to flesh diet, as, for one, in the herring-gull, and, for another, in one of the owls (*Strix grallaria*). The general relation between light and vital activity of animals is there very admirably presented.

But both these excellent writers who supply supplementary material to that which Darwin himself had before him when he repudiated his main position on "Origin of Species," have no qualifying word ; they proceed, as they suppose, to furnish facts to aid Darwin, which they may do ; but if this is in any way effected for the last phase of the master's system then they put the axe at the very root of his first phase in "Origin of Species," his own greatest error, as he had plainly to acknowledge. And all this they do—yes, they do, clever men and most learned—

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[15] P. 158.

without the slightest recognition of his absolutely changed position as regards the most essential principles; and, as though Mr. Darwin, under the irritation of counter-facts to his facts, had not, like the lizard, done something too like snapping his own old tail off.

Thus, for instance, even a writer like Professor Semper is found trying to minimise the extent to which Darwin put environment or conditions out of court in "The Origin," which looks odd in the light of Darwin's own sad confession about it:—

"Darwin, indeed, himself admitted that, in the first instance, he somewhat undervalued the selective influence exerted by the surrounding and external conditions of life; but to assert that he wholly ignored them is far from the truth" (he did not at the last, he came near it in 1859). "On the contrary, these influences constitute an essential part of his theory, though Darwin himself assigns them but a small and undoubtedly too limited part in it." [16]

Light and temperature. Yes, surely. These are essential elements. Exposure to light or the withdrawal of light is surely a very important element in what would be called conditions. Experiments with birds in confinement have been made in this direction many times, and with results the very opposite of what Mr. Darwin says, for there the effects were immediately manifest, and passed away with the first moult. Herr Schilbach, Superintendent of the Menagerie of Cassel, tried an experiment on a whole brood of goldfinches. He deprived the birds of the light of the sun, even covered the cage with a piece of cloth, and by these means he obtained very black goldfinches, with only the yellow spot on the wings—most striking; but they changed colour after moulting when the special conditions that had induced the darkening were removed. Now, in nature, there might be deprivation of sunlight—there often is over large areas—there might be seasons and circumstances in which birds enjoyed less light than others, which on this principle would imply less development of certain colour pigments and less of colour-changing chemical action consequently set up in them; and in nature, no doubt, the principle would apply in a darkening of the feathers, as certainly it does in the case of insects; and it may well be that the first moult here, too, *pace* Mr. Darwin, would induce ordinary colouration if the exceptional conditions were removed or modified. To this, indeed, may often be due in a degree the variations in colour in one species, as we have often seen, in different parts, or nearly in the same part—a difference, say, as to the tone and the extent of yellow on yellow-hammers, and of the red on bulfinches and on cock linnets. Salt, I believe (and almost by accident I discovered how at breeding time with canaries, and canaries paired with linnets and goldfinches, they will eat small doses of salt beyond what may be put in their food), and some kinds of cabbage, watercress, &c., of which, too, they are then especially very fond, has a specific effect in darkening the yellow, for when I became alive to this fact I got much darker birds, having formerly from the same parents that yielded these got very light birds—*mealies* mostly. And I may add that one of my hens—a splendid sitter and feeder of the young—which from some peculiarity of taste or constitution did not care for or even taste any green stuff save lettuce and groundsel, and was indifferent to salt, and could not be induced to indulge in either, never produced me anything but mealy birds, though fine birds in every other way, which leads me to the idea that these things have a special influence, combined, no doubt, with other and more recondite causes in producing depth of tint and darker colour. At all events, I can think of no cause to effect the change save this slight difference in the feeding and the extra supply of salt, especially at the breeding time.

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[16] "Animal Life," p. 292.

"Light—more light" were the last words of Goethe. "Light—more light" seems to be the demand of natural life, seeking to secure full vigour, health and freedom, which is the *sine qua non* of beauty. Réamur, in one of his Essays, most directly hints at this as a great law and process in the higher development of animal structures. Mr. A. G. Mayer has, in the course of his studies of insect life and structure, given the most beautiful and graceful illustrations of it in the case of the wings of the lepidoptera. Every one knows that each wing in such creatures is formed of two delicate, elastic and transparent membranes laid one upon the other, strong and nerved, and that each wing appears to be covered on its outer side with fine dust, which, after all, is composed of exquisite scales, set one upon the other, and nicely fixed together like the tiles or slates on a roof. Each scale, minute as it, is elaborately marked, grooved, or sculptured; and the lovely colours we see, say, on a butterfly's or moth's wing, are due to the massing of them together. In some cases a square inch would have more than 50,000 of these scales. Mr. Mayer has shown, by beautiful experiments, that these scales are developed from modified hypodermic cells, like the hairs of other arthropods. The colouring pigment comes from the so-called pupal blood by a series of chemical changes. The colours of the adult are not formed at once, but all pass through a series, the first of which is a dull ochre yellow. Mr. Mayer succeeded by chemical means in making pigments from the pupal blood or hæmolymph that were similar in colour to the colours of the adult insects. These pigments react to chemical agents similarly to the pigments in insects. Dull ochre yellows and drabs, such as one finds prevailing among the nocturnal lepidoptera, are the oldest; the bright yellows, reds, and greens of the diurnal forms are derived by a complicated chemical process brought about in the parts most exposed to the light.[17]

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[17] See Mr. F. C. Kenyon's Report of Mr. Mayer's investigations in the *American Naturalist* for February, 1897.

Eimer quotes Dorfmeister to the effect that, from his experiments he learns that temperature exercises the greatest influence on the colour and markings of butterflies, more especially when it acts upon them during the change into the pupa, or shortly afterwards. In many, a rise of temperature produces a lighter, more brilliant ground-colour; a fall, a darker or less brilliant,—for example, *Vanessa io*, *urticæ.* In *Euprepia caja* the red-yellow ground colour of the posterior wings is changed by a rise of temperature into vermilion red; by a fall, into yellow ochre.[18]

This is all certainly most interesting, and has a very direct bearing on the question of conditions, which act immediately and infallibly in initiating chemical changes, though the full results in brighter colour production are due to a process which passes step by step through the most subtle transformations by advancing grades of brighter and brighter hues according to the amount and intensity of the light. The brighter and more constant the light that falls on these forms the more splendid they become through complicated chemical processes set up by the light's action. The gorgeous hues and tints of the butterflies and moths of the south and east are thus scientifically accounted for, as also the duller, dingier colours of those of the north. "Melanism," in our own moths and butterflies is thus scientifically accounted for also by the lessening of the clearness and brilliancy of the sun's rays through rain, smoke, mist, moisture, shade, &c. The colouring of insects will thus be made an exact measure of the amount of direct sunlight prevailing. The ultimate *condition* thus corresponds with and has its witness in the tone and colour of the creature : its potentiality in certain directions is precisely, and from the first limited, if not fixed by *condition*, and to say, in face of this, that *condition* is quite subordinate and will not act so as to show any outward sign or mark for generations, is certainly to say what is very much in opposition to the facts. The various hues and tints of brilliant colour that come out on *Aphrodita aculeata*, or sea-mouse, are absolutely determined by the light and amount and intensity of light that fall upon it ; if the creature is never exposed to certain lights it will never show certain of its hues or colours or develop them. Illustration on illustration to the same effect might be brought from a wide circle, in which the effort towards light would be found determining in crowds of creatures the most remarkable changes and transformations.

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[18] "Organic Evolution," p. 131.

Surely, surely, these are enough to show that Mr. Darwin's great dogmatic assertion that the influence of "changed conditions," while cumulative, mark, absolutely produced no effect on a species till several generations had passed, is a generalisation absolutely false in the face of the facts; and as a piece of thinking is like so much else to be found in Mr. Darwin's writings when one resolutely and with thorough logic dissects them in this regard.

What is cumulative, in a true sense? What is it, as used by Mr. Darwin in the sentence above quoted? Cumulative is something that acts in a certain direction inevitably, and because of something that has become efficient in the subject of it—which acts more and more towards definite outward signs and marks as it continues to act—and thus produces in increasing ratio the marks and inseparable outward testimonies of its so acting. If this is so, if it is approachably so, then Mr. Darwin most egregiously failed there to realise the meaning of his own words, for "cumulative" influence thus exactly implies a certain measure of influence operating in a certain direction from a given moment when conditions are fixed; and so Mr. Darwin in "accumulates" and "cumulative" there simply contradicts his own statement that "absolutely no effect is produced on a species until it has been exposed during several generations to the changed conditions." If he had written "visible effect" that might a little have protected him.

So clear is the case against Mr. Darwin here that once again he himself had to cry *peccavi*, under the force of instances—unchallengeable instances—presented to him from all parts of the world; so tenacious was he still to reserve some apparent ground in favour of his early fallacy. The leading principle laid down in his "Domestication," that for generations absolutely no effect was produced in a species by changed conditions until it has been exposed to them for generations, when effects suddenly appeared, is grossly wrong, when in three generations in many cases the transformation is completely effected, and all the marks of earlier species completely gone, as Metzger's maizes show. In a word, the logical position is that the causes are all operating there, but there is absolutely no effect.

In his "Life and Letters" you find lying half-hidden many of the corrections and modifications and additions needed—imperatively needed—here, just as we have found there, lying half-hidden, too, the corrections, modifications, and additions demanded to correct or rectify the "Origin of Species." Now, be it noticed, these are not on small details, on which any writer may make a slip and go wrong; they are on great principles, that most directly affect the whole theory and force of Natural Selection and Survival of the Fittest. Mr. Darwin (1) would admit no changes on species that were otherwise than slow, very slow, indeed. But does this not imply definite action in a certain direction from the moment the causes are set in action? great slowness implies uniformity, as he himself over and over again said, but slowness and uniformity absolutely imply definite movement in one direction, unvarying, whether from the first outward marks of it are evident to ordinary observation or not. When cases on cases of sudden modification and change, directly due to conditions, were presented to him, he had to tell how different—very different—the "Origin of Species" would have been if he were now to begin it *de novo*, and this, mark you, not as to instances cited, but as regards the very central principle or theory of that book. Then (2) after his "Domestication" has been before the world for years, he is compelled to own that he has been quite wrong about one of the main principles of that book; that the influence of changed conditions, though cumulative, are still slow; and that *no effect*—absolutely and unqualifiedly, mark—is produced on a species until it has been exposed during several generations to the changed conditions; and this actually in face of cases which he had himself there presented. There is an axiom, "No effect without cause"; Mr. Darwin insisted there could be, and were, causes without effects.

Mr. Darwin's last state—his insistence on the slowness of operation of external conditions, as exhibiting in those subjected to them no perceptible change for generations—was even worse than his first, because it was evident then that the wonderful and unceasing effect of modifications due to change of external conditions had been effectively brought before him, and his insistence on several generations being needed to exhibit noticeable and assertive modifications, are to me exactly like the struggles of a drowning man, who will clutch at straws or any semblance of thing floating to preserve him from sinking.

IV.

The whole drift of later thought, aided as far as it can be by more thorough observation, is directly in the teeth of Mr. Darwin's long-maintained and *never really completely abandoned* theory in the "Origin of Species"; and this is mainly summed up on two grounds: (1) that nature may and does take sudden and unaccountable leaps, and (2) that, instead of natural selection being even remotely independent of conditions, natural selection is absolutely only *another name for organisms regarded as adaptations to environment or to outward conditions.* This is Mr. Herbert Spencer's view—a view for which we are indebted to him alike for original thought in his full and clear perception of it and for wide and felicitous instance in illustration of it. The whole purpose of the fine essay, "The Factors of Organic Evolution," lies here. Natural selection is only one of three factors, the first being inherited effects of use and disuse, and the third and most prevailing, the "direct action of environing matters and forces." Here is the thing put shortly in his "Principles of Biology," chap. v.:—

"Every organ and every function of living beings undergoes modification to a limited extent under the stimulus of any new conditions, and the modification is almost always such as to produce an adaptation to those conditions."

Dr. Weismann, who regards himself as quite a logical Darwinian, yet can write thus destructive of some of the principles which Mr. Darwin himself never did rightly forego:—

"We must assume natural selection to be the principle of explanation of the metamorphoses, because all other *apparent* principles of explanation fail us, and it is *inconceivable* that there could be yet another capable of explaining the adaptations of organisms without assuming the help of a principle of design. In other words, it is *the only conceivable natural explanation of organisms regarded as adaptations to conditions*.''[19]

But then certainly this is not the natural selection of the " Origin of Species," if it is approachably that of Mr. Darwin's later phase, and I say, for clearness, and to prevent gross mistakes and misleadings, Dr. Weismann should have said so.

Change of external conditions, in fact, is what lies at the very base of natural selection, and without such change natural selection is absolutely inconceivable. Mr. Darwin himself finally admitted this, as we shall soon see. The incessant, and it may be unnoticed, steps in the change may be very slow and gentle in some circumstances; they may be very marked and strong in others; and it is only when we have mastered these—more especially as bearing on the modification of the most sensitive parts, the sexual organs (in birds that is very noticeable and accounts for much)—that we can in the least estimate the *rationale* and extent to which natural selection has been in evidence. The one form of knowledge is absolutely dependent upon the other. And yet Mr.

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[19] *Cont. Rev.* vol. xliv., pp. 327, 328.

Darwin for years and years went on trying to regard the superstructure as absolutely apart from its foundation. Dr. E. Russel Wallace himself is not consistent here, for, while in many places he speaks of changes of conditions universal and *unceasing—unceasing*, mark—and of changes of climate which are continually occurring, yet he also refers to conditions which do not perceptibly vary from year to year or from century to century. But this last, Dr. Wallace must pardon us for saying, is rhetorical rather than scientific; because, if conditions are fixed and invariable, or very closely approach it, natural selection would also necessarily be without record, its results so very slight, insignificant, non-significant Professor E. Ray Lankester would say; for why should there be useless and uncalled for variation in any aspect of it? Variation on the laws laid down only comes by adaptation to changes. Mr. Darwin makes no exception when he says that "plants and animals have to struggle from the hour of their birth to that of their death for existence,"[20] which surely is the direct opposite of what he had said in the "Origin of Species," that we may console ourselves with the thought that the struggle is not *incessant.* The struggle will be measured precisely by the changes in condition looked at from the cosmic and geographical sides; if, indeed, it may not be that there is a delusion in looking at the same instant from both sides of Mr. Meldola's line of mean variability without knowing it.

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[20] "Domestication," ii., p. 233; 1st ed., p. 61.

The whole business, indeed, of making Natural Selection start away on some set of arbitrary variations presumes a settlement of conditions under which types have been fixed and have long, or, at least, for a considerable lapse of time, existed—that is, definite species have been recognised. Then comes change, producing variations slight and hardly noticeable, on which, as bases, Natural Selection can work. Never did we before have to deal with such utter mechanical and illogical paradox. The assumption of a period free from changes of condition, with fixed types, confirmed and ironed down "species," is the merest pretence. There is not, and never has been, and never will be anything of the kind, which these very gentlemen admit, and fully admit, the moment that they are launched out on the wide sea of observation. Why, is not the very idea of the great slowness of which Mr. Darwin made so much, and the continuity of development, directly in the teeth of this general statement? Of course it is, and they cannot get away from it. Not even the wonderful logical craft of Prof. E. Ray Lankester can quite accomplish it with the assertion of the "doctrine of the origin of species by the Natural Selection in the struggle for existence of *non-significant congenital* variations." "Non-significant"; and the man's very words—in their clear and natural meaning—bear distinctly that from his point of view and that of his

Natural Selection, they *are* significant, and significant enough for him to dogmatise. And "congenital." Here, what does that mean?—or does it mean anything? I should like the most logical Professor E. Ray Lankester to tell me, and then I shall perhaps have another little word to say to him. They are all so logical, these fine fellows, so philosophical—deeply philosophical, and scientific, as well as see so far—so very, very far! And some of them draw such fine lines with it, "too low in the zoological scale," you know. "Non-significant congenital variations." Well, and that makes the measure of their significance to be determined absolutely by our powers of perception of them—surely, not what should come of the doctrine of slowness and uniformitarianism. And if all such variations are congenital, how about the unceasing and universal change of conditions, &c., &c.? Are there no variations whatever wrought upon the developed creature as well as on the progeny on being born or brought to life?

With all respect to Professor Ray Lankester, it is due to myself to say that his *non-significant* congenital variations become significant, and significant in the very highest sense, the moment they are perceived and presented to us as actual facts—they are, on his own theory, cumulatively to us the proofs—and the only proofs we can have, of the "struggle for existence" on which the theory of natural selection bases. If they remain *non-significant*, that is really a contradiction in terms. I say the theories of "natural selection" and "survival of the fittest" can be best established only by the very translation of a certain body of these facts into absolute significance; otherwise, there is no true basis for any generalisation in natural history whatever. Professor Ray Lankester's definition surely thus explodes of itself, even by verbal contradiction and exclusion.

Dr. Ray Lankester's "*non-significant* congenital variations," in fact, are the logical analogues of Mr. Darwin's very remarkable operative causes (non-significant in his idea, truly), which for several generations have no effects—no visible or appreciable effects whatever.

Mr. Darwin failed exactly here—he laid down certain rules or laws and tried to make all facts square with these. He found as he went on that even he couldn't work it. Facts—the only facts he is said so completely to have mastered—were too much for him; and that they were so, is attested by his frequent changes of position, which may be reduced to three great and specific changes: (1) from the theory of pure natural selection, practically independent of conditions, by slow, continuous and almost imperceptible degrees in "Origin of Species," to the (2) admission of exceptional cases in "Domestication," and (3) to the frank recognition of outward conditions as the most potent of facts in "The Descent of Man," "Fertilization of Flowers," &c., though, to the very last, he was fain to fall back too much on "slowness," declaring that no change in species, as species, was apparant for several "generations"—at least, as if at a certain point, quite arbitrary, the forces that had been really at work all the time, suddenly shot out some *definite* mark or sign so that it was assertive in the eyes of all—a most mechanical element recalled, and potent to the end with Darwin. Even here Professor Ray Lankester's "non-significant congenital variations" became significant, which says much at once for observational powers and for logical discrimination in all concerned—for here Darwin and Ray Lankester join hands in the attempt to conserve and to press on the minds of other men what is nothing but a patent absurdity by which, alas, the minds of men are made the measure of nature and all the possibilities of nature—the results of which, carried to application, is found in such a thing as the foolish, foolish, ill-informed writing of Mr. Romanes about nature not having counter-instincted little birds against the cuckoo because it was not worth her while to do so, the deposition of a cuckoo's egg being, comparatively speaking, so exceeding rare an event (non-significant, really), or the arbitrary laying down of a law of exclusion by the easy resource of "too low in the zoological scale," or Professor Ray Lankester's very acute and logical "non-significant congenital variations."

Dr. Lankester has also what we confess is to us rather a wonderful supposititious instance of the theory of natural selection acting on congenital *fortuitous* variations. I pass over the "congenital" here to dwell on *fortuitous.* In what sense does Dr. Lankester use that risky word here? Does he mean that these "congenital variations" were fortuitous because natural selection had not acted to produce them, but only was to take them up and act on them when they had been produced, because, in consistency with his doctrine, if it had done so they would not and could not have been *fortuitous*; and if fortuitous in any natural meaning of the word, then they were pre-natural-selections, with another "very fine and large" line drawn through at a certain point. I like these definite lines, but this one does not really make in Dr. Lankester's favour, after all, when you closely attend to it; for his fish or arthropods swept into a sea-cavern—some of them with so imperfect eyes that they could not see their way even towards the light (which the merest jelly-specks can do, by-the-by), to get out again, perforce remain, breed, and get blind, and their descendants blinder still.

But the process would be much slower inevitably than Dr. Ray Lankester allows for. Besides, as Cope well shows, the eyes of certain cave-fish are developed in the egg and in the young, though they disappear in the adult; so that on the doctor's own supposition, the young might see their way towards the light if the adults could not. Cope points out, too, that cave-insects have long delicate antennæ and limbs exceeding those of their out-door relations by much.[21]

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[21] "Factors of Evolution," p. 243.

And the doctor says a case of natural selection would thus be effected. It has been well pointed out more than once that this is not a case of *natural* selection at all in his sense of it. It is not pure natural selection any way, for here, by sudden violence or cataclysm (which pure natural selection excludes, as he says Lyell excluded it in geology) a whole group of animals are swept clean away from their natural habitat and conditions; and what I *should* like personally to ask Professor Ray Lankester in presence of a select group of his students and admirers is this (and since I cannot ask it there, I now ask it of him here): "Is this a case or is it not, illustrating the doctrine of the origin of species by the natural selection in the struggle for existence *of non-significant congenital variations ?*" The very thing (sudden changes of condition by cataclysmic violence which pure Darwinism shuts out) is here posited as an essential and necessary cause and element, and so far I decline to go even with the "Encyclopædia Britannica."

VIII.

Mr. Darwin is highly praised for his patience, his caution, his candour, &c. No doubt his great characteristic was a genial patience and care in observing facts, in contrasting, estimating comparative processes, and in making record ; but the moment he came to draw inferences—to deal with principles, laws—then, indeed, he was often all at sea; then he was, now too hasty, too bold in rushing to conclusions; and, again, sometimes he seemed a little too much inclined to put aside an inconvenient or rather exceptional fact till he could do so no longer. He clung to his theories and speculative generalisations with a pertinacity that was surprising in association with so much candour, openness, frankness of disposition; and what I cannot help in some degree attributing this to was his misfortune, as it has been the misfortune of too many men in England, to be hailed as great thinkers, vast original initiative minds, when they really were not. Mr. Darwin was no more than a careful observer, a patient classifier. His great ideas as speculative theories had all been one way or another suggested before by Lamarck, Buffon, Oken, Bastian and Chambers in Scotland. His chief glory was illustration, systematising, which he overdid and went wrong over. He was really so poor a thinker, with so little power of abstract thought, that to his "Life and Letters" must every thorough student go to find the true commentary, corrections, qualifications and emendations on his books—to see how often he had to shift his ground, yet would not completely leave it, always trying to preserve a bit of standing space on a corner of the old area, vast tracts of which had been already, and on his own confession, submerged around him. So, he seems to us, in the end, a man struggling to make his works consistent by aid of entries in his diaries and in letters—imposing thus, it may be in part, unconsciously, on those who follow him, a vast and not seldom an irksome labour—so vast and so irksome, indeed, that few there be that persevere in and master it. To the few who do, Mr. Darwin will figure as a great scientific collector, as a man of wondrous eye and powers of grouping, but not by any means as a great thinker, for which he had the misfortune at an early age to be taken, with ceaseless and loud celebrations of it, and in so far he was spoiled for the yet greater, if in one point of view the humbler, work that he might so well have accomplished.

PART III.

SPECIAL ILLUSTRATIONS FROM "CERTAIN SECONDARY SEXUAL CHARACTERS" AND INFLUENCES OPERATIVE ON THEM, &C.

DEER, DEER'S HORNS, AND LONGEVITY, &C.

I.

There has been a great deal of discussion about the development of deer's horns in relation to modification of the sexual organs, and the cause, or causes, of the malformation of horns in deer. Mr. Tom Speedy, having pointed out that after maturity, in the twelfth or thirteenth year, the antlers gradually fall off in size and appearance, he proceeds to account for the excessive size of the antlers in certain breeds by saying that no doubt high-feeding is the cause, as they are never met with so large in the wild state. He also concludes that the malformations of antlers —single or unicorn in some cases, at different angles in others, with different curves in the stem in others, and in yet others, with one antler growing normally, and the other growing downward—may generally be traceable to close breeding or to former wounds. We should say that the former is generally the cause, pointing to modification of the sexual organs. It is well known that in castrated animals the horns either (1) wholly cease to appear, or (2) are of the most rudimentary character, or (3) are never again shed.

The almost incredible sensitiveness of the sexual organs, in all creatures, to any change or exceptional stimulus on any part of the system, has only been recently *fully* realised. Mr. Darwin awakened to it late, and never fully realised it; Haeckel has devoted much time to it, and, if he has not solved the problem, has, at all events, set others on to scientific and thorough examination of the facts connected with it. "Every change," he says, "in the sexual organs powerfully reacts on the rest of the body; so, on the other hand, every important change on another part of the body must necessarily, more or less, react on the sexual organs. This reaction will, however, only become, in many cases, perceptible on the formation of offspring which arise out of the changed generative parts."[1]

And again:—

"Certain influences act upon the male organs of propagation only, affect the structure of the male descendants, and, in like manner, other influences act upon the female organs of propagation only, and manifest their effects only in the change of structure of the female descendants. This remarkable phenomenon is still very obscure, and has not yet been properly investigated."[2]

Moreover, length of life itself is largely dependent on certain restrictions or indulgences of the sexual appetite viewed in certain aspects; in fact, the rule with regard to various lower forms of animals—that the moment of reproduction is also the moment of death—in a modified way, still largely prevails through nature; and Dr. Ray Lankester has very skilfully made a great deal of this principle in his admirable, able, and most compact book on "Longevity." Traces of the same law he finds in the very highest ranks of existence, and among the most highly differentiated creatures—in fact, the higher you go in this the more illustrations you find—something like a moral idea being developed in the process through individual care of the young, and long association with them on the part of parent or parents. Among birds smaller broods go with a greater longevity. Among fishes, again, we see that with those which give personal attention to the young, the bulk and number of the young are immensely reduced. The pipe-fish, for example (which carries its young in a pouch, and the tube-mouth, which does the same—only the father in the one case and the mother in the other case), hippocampus, and aruis of the Amazons. He also finds corroboration of the principle, in mules and cut animals, that generative expenditure antagonises longevity. Many exotic creatures—birds and mammals—in confinement or under artificial conditions, live long in this country. For example, it is a notorious fact that cockatoos and parrots do not breed in our country, and the question might well be put—Can this have anything to do with the very long age they attain?

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[1] "History of Creation," i., p. 247.

[2] *Ibid.*, i., p. 230.

Is there any reliable record of the years parrots attain in their natural habitat and in freedom, and are they more or fewer than parrots live here in confinement?

II.

Every one who has had to do with birds on any extensive scale knows that birds put to breeding do not live nearly so long as those which are not, the treatment being otherwise exactly the same. Canaries, for example, if put to breeding will not live longer than from twelve to fourteen years, however carefully treated and done by, whereas those not so will live to about twenty years, the hens, strangely enough, tending to have about a year's longer life than the cocks. On this subject Mr. James S. Gould, a great authority on canaries and cage birds, writes in "My Canary Book":—

"Canaries well treated will live from fifteen to even twenty years, and will sing to the last; but those kept for breeding purposes seldom live longer than ten years."[3] So that in our remark above we have understated it.

The fact that capons are so definitely operated on with a view to the table deprives us of the comparisons that might otherwise be available, but it is not unreasonable to say that by analogy we might expect the capon to live several years longer than the entire cock. Stallions, I understand from good authorities, do not live so long as cut horses, notwithstanding that the cut horse is cut for the very purpose generally of exposing it to hard work and toil and injuries of many kinds. And there is, besides, the extraordinary fact that stallions generally, on the contrary, suffer from lack of exercise rather than from any exertion put upon them. Practised breeders of horses of all kinds have become alive to this, and are now more inclined to act on the knowledge than they once were. Major John Humfrey, in his valuable book on the "Breeding of Horses in India," says:—

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[3] P. 14.

"Why Government stallions should be so uncertain I am unable to do more than guess at. My own belief is that from want of exercise the horses get soft and unfit for a purpose which requires the utmost bodily vigour. I am of opinion that every one of the stallions would be better for regular, steady work mounted."[4]

And the Earl of Suffolk and the Duke of Beaufort, in the Badminton volume, say that many stallions are spoiled for want of proper exercise.

So, from these instances and others that might be cited, it is clear that virile strength in the horse can best be maintained by some steady degree of work, and this, I believe, could be substantiated by the experiences of some in countries where entire horses are more used for purposes of service than with us. And some justification for this will be found in the fact that in the wild state horses fought for the females as the deer do ; and, indeed, we are told by Sidney (in the "Book of the Horse," p. 148) that " the great horse-breeders of California adopt a plan copied, we presume, from their Spanish predecessors. They pick out a stallion and about forty mares, corral them, that is, keep them in a pound for a certain number of days, and then turn them out to the open pasture plains. The mares then keep to the horse, and the horse permits no intrusion, at least until he has been defeated in pitched battle by some rival sire."

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[4] P. 14

The case of the camel is very peculiar. It has a rutting season exactly like the deer, but it has so long been domesticated that it might be expected the trace of this would have been lost. But it has not. The camel reaches an age of from forty to fifty ; it is full grown at fifteen. The period of gestation is one year, and there is only one calf, which is weaned at about a year old. Lieut. Massoutier says that in Algeria nearly all the camels are geldings—only a few of the finest stallions being kept for stud purposes in the proportion of about one to fifty. A certain proportion of the females also have the ovaries removed and are used to carry loads, but this only to a limited extent. As breeding and milk-giving animals they are far more valuable. A female should not be worked for at least three months before giving birth, and not for at least three months after it ; but this is not acted on, and consequently there is great mortality not only among the young, as the mother cannot both work and give milk, but among the breeding animals themselves a great many females die before reaching the twenty-fifth year, and, taking everything into account, it may be said that in the case of the camel, too, those that are devoted to breeding do not reach so long an age as those which are cut, notwithstanding that they are often worked very hard.[5]

If such be the case—if conditions tending to effect the system—food, for example, or change of food, acting directly on the system and indirectly or less directly on the sexual organs, then to a certain degree Mr. Tom Speedy's two separate causes—high feeding and close breeding—really resolve themselves, after the first step, into one—physiologic action to one effect on the generative organs. When he quotes Mr. Parker Gilmore to the effect that he found among the stags of Vancouver Island many with malformed antlers, and that this he accepted as an unquestionable proof that their surroundings were not exactly such as nature intended them to enjoy—we may well be sure that the generative organs were in it, if the results were too subtle to escape notice. It is not at all likely that all these stags would have been injured in other parts of the body in much the same way.

Mr. Darwin is very decided that "It does not appear that a change of climate, whether more or less genial, is one of the most potent causes of variability";[6] but he is clear on this that "Animals when first confined are very liable to suffer in their reproductive systems,"[7] a result, however, which is to some extent, if not largely, compensated, in many cases by time—in a few years not a few animals have recovered and bred successfully.

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[5] A. G. Leonard, "The Camel," pp. 54-60.

Mr. Darwin accepts it as proof that the sexual functions have been disturbed in the common linnet *(Linota cannabina)*, that it does not acquire the fine crimson colour on its breast.[8]

The same may be said of yellowhammers, which have much less yellow on the feathers, and that yellow of a much fainter kind, such as we have ourselves observed repeatedly, and there are many similar instances among the birds. I remember Mr. John Hancock spoke of instances of linnets without the red breast breeding in the north; but though this was the case the progeny may have suffered.

III.

Let us now look at some of the facts presented to us by careful observers, students of natural history, and sporting men with regard to the deer and the shedding of horns, &c.

Mr. Whitaker in his "Deer Parks" does, indeed, thus bring Mr. Speedy's two causes into one in his generalization as follows:—"The size of the horns depends much how deer are fed in the winter, and whether they have also good pasture while the horns are growing, and *are not too much inbred.*"[9]

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[6] "Domestication," ii., p. 244.

[7] *Ibid.*, ii., p. 143.

[8] *Ibid.*, ii., p. 140.

Dr. Palk Collyns, in his excellent work on "Stag-hunting on the Exmoors," gives it as his opinion that after a stag has passed his prime, the horns decrease in size and form until the horn deteriorates, and nothing but an upright or single spine like that appearing in the three-year-old deer, is visible.[10]

Mr. Lydekker gives us this record:—

"Of four well-fed red deer kept in captivity at Blair Athole, the following record of their antler-development is quoted by Mr. Gordon Cameron:—'One of the stags continued to improve till he was nine years old, after which he remained stationary till fifteen, when he became diseased and was shot. Two of them went on improving till they were eleven years old, and then remained stationary till they were sixteen, when they became diseased and were shot; the fourth went on improving till he was thirteen years old, when he was killed in a fight.'"[11]

Mr. J. G. Millais gives from twenty to thirty years as the limit of a stag's life, but eighteen, he asserts, is as much as a park stag ever attains.

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[9] "Domestication," p. 9.
[10] Quoted in Badminton Series, "Hunting," p. 40.
[11] "Deer of all Lands," p. 9.

Another good authority makes this remark :—

"It is curious how slight a wound will cause a bad growth of horns."

But a link of influence is clearly lost in such a statement—the direct bearing of growth and form in horns being proved to have so immediate an effect, if a subtle one, on the generative parts. The sentence would better have been : "It is curious how slight a wound may modify the sexual organs so that a bad growth of horns is the result." Major-General Hope Crealock has given many cases in which a slight injury to the sexual organs has resulted in abnormal horn-growth, while yet propagation was still possible ; and he cites certain cases of deer where the castrating process had not been complete which presented very peculiar phases altogether—corresponding in the case of deer to the cases of certain imperfectly maimed eunuchs of which Sir Richard Burton has told us.

Whitaker writes :—

"The effect of castration on the growth of horns is very curious. If a buck be castrated when his horns are off, he never renews them; if when on he never sheds them. If cut on the right side he grows a horn only on the left, and *vice versâ*. I am informed also, that if the operation be performed while the horns are 'in the velvet' they always remain in that state ; but this I do not know for a certainty."[12]

A very curious question here arises about one-horned stags:—It is quite clear that such stags can propagate—if they do so, as we believe they can, in spite of partial or incomplete castration, or in spite of injury to one side of the testicles, then are their progeny imperfect in the same way as they are, or does the one-hornedness persist, while what was considered the cause of it, does not?

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[12] "Deer of all Lands," p. 10

From Hofacher's account it appears that a one-horned stag was seen in 1781 in a forest in Germany, in 1788 two, and afterwards, from year to year, many were observed with one horn on the right side of the head.[13] In this case were the one-horned stags of later years descended from the stag of 1781 or what? and if so, can it be generally said that such wounds as affect the horns and sexual organs, be inherited and give rise to new variations?

Major-General Hope Crealock does something to make this point more clear—the stag which is injured while "in velvet" never really gets rid of the velvet, but it dries and hardens—the animal not then caring to rub or clean it off by friction on trees, &c.

He writes:—

> "It is believed that a stag which gets injured in his generative organs, either accidentally or by operation, will never shed his horns if he has any at the time; or if the injury should have occurred when he has no horns, he will not grow new horns afterwards. But there are exceptions to this in cases where the injury may have only been partial. Mr. John Fowler gave me a curious instance of this which came under his own observation at Braemore Forest in 1890. He killed a

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[13] Darwin, "Domestication" ii., p. 228.

fair stag in excellent larder condition near the middle of October—quite at the end of the season; he had his horn still encased in velvet, but the velvet was quite tough and hard. On examination he was found to have been injured in his generative organs; whenever this occurred he must have been in velvet, but had never cleared or shed his horns afterwards. It was objected that had this been an old injury the stag would have worn away the velvet, but I, on the contrary, think that his horn was mature beneath, so that he never shed it, and the velvet dried up and became hard like leather; certainly, the velvet will remain on a horn not quite mature after death, as I have one, and it is quite dry and hard as this is stated to have been."[14]

Mr. Scrope says:—

"If a hart is cut when a fawn he never will have horns, and if he is cut when five or six years old, after his horns have attained their full growth, he will never drop them, and if he be cut when he has dropped them they will never be renewed. . . . Yet I once killed a very large fat hart on the top of Ben Dearg in the month of September which had not been cut, and still had no horns at all."[15]

Mr. Lowndes, of Castle Combe, wrote to Mr. Whitaker:—

"Mr. Shirley speaks of some way of castrating deer so that they will put up fair-sized horns; but I have never known a havier here to grow horns. I killed one, however, a few years ago that had an excrescence covered with velvet about the size of an orange, on its head. It was soft and spongy. . . .

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[14] "Domestication," p. 32.

[15] "Art of Deer-stalking," p. 7.

Even in the month of October when the havier would have to be killed the lump was still quite soft, and had become no bigger than an orange."

Dr. Caton, of America, has paid particular attention to deer in his own country and in some other places. He tells us that several American deer castrated by him never so matured as to lose the velvet.[16] Dr. Caton also tells us about finding hinds with horns in cases where as in our deer the females are normally hornless. He says:—

"It may not be improbable that these antlered does will always be found to be barren. My observations upon the effect of castration on the growth of the antlers of the buck show that there is an intimate connection between the reproductive organs and the growth of the antlers, and so it is not unreasonable to suppose that the phenomenon of antlers upon a female deer may arise from some peculiarity of the ovaries, or some other of the genital organs."[17]

Dr. Caton does not say here that these horn-growing hinds were old; on the contrary, when he speaks of some peculiarity of the ovaries or some other of the genital organs, we are justified in inferring that they were at such an age as otherwise they might have bred. Had these cases been only known to Mr. Darwin when he wrote "Domestication" he would have had to modify and to expand the following sentence so as to have made it inclusive of later facts and consistent with them :—

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[16] "American Antelopes and Deer," p. 189.

[17] Caton, "The Deer of America," pp. 232-3.

"The females of two kinds of deer, *when old*, have been known to acquire horns; and, as Hunter has remarked, we see something of an analogous nature in the human species."[18]

The explanation here, "*when old*," is ruled out in so far as concerns the possibility, though, no doubt, the causes in the broad coincide as to their results not invariably being due to old age alone—malformation or accidental injury, or congenital defect, have exactly the same effect, we take it, as proved by Dr. Caton's instances. Now, here is the pressure of the problem. Other causes than old age produce the hornedness in female deer. What are they? When this is established beyond doubt our curiosity may be more fully satisfied; but as evidence stands now we have assurance that certain causes in young hinds will produce the horn-growing quite as certainly as in some cases old age does it.

Mr. Alston in his article "On Female Deer with Antlers" (*Proc. Zool. Soc.*, 1897, pp. 296-9) cites two cases in our country of female deer with horns which were either suckling young or were with young—one on the estate of Sir J. Fergusson, of Kilkerran, Ayrshire, which was shot on January 5th, 1879; and here the horns were in velvet. On the day she was shot she was accompanied by a last year's fawn, and her nipples showed she had recently been suckling. Another was on Lord Egremont's estate, the skull of which is in the Royal College of Surgeons. Lord Egremont said that this deer was an old and uncommonly large female, with two young ones in her. Other instances are given from Germany and America, and reference might have been made to the large female deer with horns which Scrope refers to in the Hebrides. Mr. Alston winds up thus :—

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[18] "Domestication," ii., p. 26.

"If horns were at first common to both male and female, the problem appears to me to be capable of a more satisfactory solution. In the males they would naturally be further developed by sexual selection, and in the females the strain on the constitution would tend to their reduction, or even elimination, this strain, as Mr. Darwin himself has pointed out, being much the greatest in the *Cervidæ*, in which the weapons require to be renewed each year. That they should be retained (usually in reduced size) by the females of most of the forms with the non-deciduous horns appears therefore to be natural, while their retention in the female of the reindeer, and their occasional abnormal development in those of other little specialised deer, is no more than we should expect on the doctrine of heredity."

And yet, in face of all this, we find in Mr. Lydekker's valuable and beautiful book "Deer of all Lands," a very different conclusion from what these facts would seem to lead to and the inference drawn by Mr. Alston. He tells that there are no antlers whatever in the primitive types of deer and that the first antlers are invariably of the simple forked type; and, taking these facts in connection with some others, he concludes :—

"This is certainly very strong evidence against the theory that sexual selection can be regarded as sufficient to account for the origin and purpose of the frontal weapons of this group of ruminants. And in the opinion of good judges they were first weapons of offence and defence against beasts of prey and as such were common to both sexes, but subsequently lost in the females under such conditions as rendered them protected substantially from powerful enemies." And he cites Dr. Caton's instance to the effect that—

"Among a herd of wapiti in captivity in Ottawa, the mastership was held by a large stag with small antlers, while a smaller stag with much larger antlers was kept at a distance."

Mr. A. Gordon Cameron tells also that among Scotch red deer antlerless or bald ones are a match for horned stags.

How, then, account for the transformation of mere instruments of defence borne alike by male and female into accepted secondary sexual signs or marks? how account, also, in such cases as have been named, in view of bold generalisations under sexual selection, for antlerless or "bald" stags taking and keeping the place of supremacy? and lastly, how is it that precisely in the degree that such weapons have become less and less necessary or useful in view of defence or offence (Mr. Tom Speedy declares that the largest and finest horns are borne by highly and artificially fed deer) they should in one sex have become more and more distinctive and ornamental if not the accepted sexual secondary marks?

And yet, in spite of this assertion of Mr. Tom Speedy above, antlers and skeletons of full-grown stags have been found in lochs, especially in Forfarshire, and peat-mosses in Scotland, "with horns infinitely larger than any," says Scrope, "which I have ever seen on living animals of the same species";[19] and as the country must then have been wilder, they were living a more natural life then. Scrope adds:—"At first sight this seems difficult to account for," and so it is. And perhaps it would be felt to be all the more difficult of explanation if it could be proved that in the peat-moss and loch-bottoms deer the females were horned as well as the males. Mr. Scrope writes further:—

"The horns of one of the largest stags killed by the Duke of Athole weighed 13 lbs. 2 oz., still very inferior to some found in peat-moss."[20]

These facts do much to complicate matters and certainly give pause to our accepting absolutely the assertion that fine heads are due solely to artificial feeding—that may be a factor, but it is not the only factor, unless we are to admit the presence of changed conditions to such an extent that influences which now go to determine sexual force and to develop secondary sexual marks were not then in existence, or, at any rate, to any large extent operative.

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[19] *Vide* "Lyell's Geology," ii., p. 259.

[20] Scrope, p. 9.

Is it possible that the development of horns to such proportion in the male deer and their abortedness in the females is a result of man's forcing this class of creatures into systematic polygamy as he has forced ducks and geese into it with peculiar changes upon them, and that secondary sexual marks are the witness of it? In a word, are we here to trace, first the unconscious influence of man in modifying such creatures in most important respects without knowing it or intending it, causing, by a change in general treatment just as *intentionally* in the case of game-cocks to produce vastly enlarged spurs, &c., the transformation of mere weapons of defence into ornamental appendages to one sex, which appendages become associated with sexual power and possibility, and secondly, later, of direct choice and forcing certain sexual habits by high feeding on the males?

This is a subject that would afford scope for so much observation, inquiry, comparison and argument, that we regret exceedingly Mr. Darwin did not thoroughly tackle it. It wants someone like him to apply himself thoroughly and earnestly to it now and make it clear, for if Mr. Lydekker's facts are right and unquestionable, here we have a case of frontal weapons at a certain point or during a certain era taking on wholly a new character; instead of being primarily necessary and useful in both sexes for defence, becoming what is apparently expressly sexual and in a most remarkable manner answering, and answering directly, to the condition of the sexual organs.

Beyond all the influences we can reckon on, there are clearly others which are beyond us—entirely beyond us. We know that deer, though in some respects strong and enduring, in others are most sensitive and easily affected. Mr. Millais says :—

"Extremes of climate affect deer very much. A continuous wet season upsets their stomachs, and a very dry one, besides being bad for calving, drives them to the mountain tops, where, though they escape the flies, they find only poor and wiry grass, the consumption of which generates only inferior heads." [21]

The colouration of the horns proves more than appears at first sight. Mr. Millais dwells on the peat colouration of horns and the habit of the stags, in all such places as peat is accessible, colouring the antlers in peat. He seems to think that this may in so far have a protective aim; but that to us does not exhaust the significance of it, though it certainly does, to some extent, mix up merely sexual with useful and protective. Here, for example, is an instance, derived from experience, of results, the true causes of which have not been reached :—

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[21] Scrope, p. 52.

"The climate of Great Britain is probably unaltered since the landing of Cæsar, and that this country was probably the habitat of the reindeer, would seem to indicate that the reintroduction of these as denizens of wilds would be successful. Such attempts, however, as have been made in this direction, in Mar Forest, by the Earl of Fife, and by Mr. Robert Traill in Orkney, have not been encouraging in their results, in spite of the abundance of reindeer moss (*Cladonia rangiferina*), which grows freely in Northern Scotland and the Orkneys." [22]

On the other hand, and as certainly, in some conditions and under certain circumstances—injuries in wild life, intentional operation in domestication, cause traits properly belonging to the female to be acquired by the male; capons take to sitting on eggs, and will bring up chickens, and Darwin points out that "the utterly sterile male hybrids from the pheasant and the fowl have a delight in watching when the hens leave their nests, and taking on themselves the office of sitter." [23]

In the case of several wild birds, one of them the Californian quail, in captivity the cock-bird does the main share of incubation, whereas, in the wild state, it is exclusively done by the female.

More than all this; in certain cases the sitting bird undoubtedly communicates something of her own character even to the eggs and young of other species, which may be given to her to incubate.

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[22] Millais, p. 21.

[23] "Domestication," ii., p. 27.

Mr. Sadler, a gentleman who paid a vast deal of attention to breeding in many kinds of birds and animals, wrote :—

"It has been well known that birds bred from the purest game-fowls will, if hatched out under a common-bred fowl, partake of her nature, and only fight, like dung-hill fowls, so long as victory seems assured to them."

He also told a friend that he once possessed a tame raven which he induced to sit upon, and hatch out, some eggs of the common fowl. In consequence, the breath of the young chicks "stank horribly," like that of the raven which had hatched them.[24]

This is a part of the subject that has not been, as yet, sufficiently attended to. Mr. Darwin does not, in any thoroughgoing way, if at all, touch it. He does not in the least refer to it where we should expect him to do so. But we know that if a hen sits on ducks' eggs, in such circumstances that the ducklings cannot see water from the furthest points to which she leads them, they will probably grow up to dislike water as much as hens do; and here at once you will have a variation due alone to the bird that sits. A friend of mine in Essex is, at the moment I write, trying coots' eggs and water-hens' eggs under ordinary farm-yard sitters, as far as he can, to test this matter, and in the end of the year I hope to be able to tell something of the results. In how far the cuckoos may have been gradually modified by the effect on them of the sitting bird is a question that well may arise, and the thought of it decidedly goes in favour of the idea of the hen cuckoos preferring to lay their eggs in the same nest as that they came out of.

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[24] Day, p. 142.

Our own experience fully confirms the statement—a hen bird, which only died a few years ago of old age, for some years sang as well as any cock. "Old hen canaries which have done breeding often sing the whole year round like cocks, only their notes are not quite so loud and varied as those of a cock bird." Waterton gives a case of a hen which had ceased laying, and had assumed the plumage, voice, spurs and warlike disposition of the cock; when she was opposed to an enemy she would erect her hackles and and show fight. Mr. Hewitt gives analogous cases with hen pheasants; Mr. Darwin quotes these cases, and then goes on to say:—"We thus see that in many, probably in all cases, the secondary characters of each sex lie dormant or latent in the opposite sex, ready to be evolved in peculiar circumstances."[25]

The whole question of sex, and of certain *latent possibilities* in sex is here opened up. We know that the milk glands in man, as in many other animals, are present, though aborted by disuse, and that under certain extraordinary circumstances they might be stirred into action. "Degenerate women frequently have supernumerary milk glands arranged on the abdomen, as in some lemurs, while males may have supernumerary breasts of either male or female type. These breasts may be represented by nipples alone. In either sex arrested developments of the face, middle ear or palate often co-exist with these supernumerary breasts."[26] The wall that apparently divides sex from sex is thus demonstrated not to be impassible absolutely under certain circumstances and in certain conditions, the one sex may develop the most characteristic and essential marks and tendencies of the other, and the possibilities of this same always lying latent in them is attested by certain organs in an aborted or merely rudimentary condition. When, therefore theories, political or other, are founded on that assumption, they may be safe enough and correct enough for purposes of practice, but, scientifically the distinction is not so fundamentally or absolutely valid as might appear at first sight, and this not merely in regard strictly to what are merely secondary characteristics.

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[25] "Domestication," ii., p. 26.

IV.

Then there are some very remarkable facts which, so far as I am aware, no one has as yet done anything scientifically to explain, that certain orders of creatures in confinement will not breed at all with their own species, but will to a certain extent breed with species nearly allied to them. Mr. Darwin, in the second volume of "Domestication," has specially referred to some of these, but he certainly does not succeed in giving any in the least feasible explanation of the facts. Remembering Haeckel's remark that "certain influences act upon the male organs of propagation only, and others act upon the female organs of propagation only," may we not find in this some explanation of the remarkable fact that some creatures in confinement will not breed at all with their own species, but will to a certain extent with species nearly allied to them. My theory would be that in all these cases either the males or the females had undergone such modification that though copulation might or might not be possible, yet fertility was not possible. Mr. Darwin has in "Domestication" drawn together a great many facts bearing on this subject. In this remarkable fact there is a provision for a wide margin of variation in certain areas, and a full and complete study and presentation of these would form a fine supplemental chapter to Mr. Darwin's "Domestication." This one point itself demands, and would well reward a whole book—the question of hybrids, and the place they hold and the lessons they teach. Some of these lessons are quite counter to Mr. Darwin's presumptions, and in their own way specially prove the truth of the striking words of Haeckel. Dr. Brooks, in his "Heredity," has dealt with some.

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[26] Talbot, "Degeneracy," p. 269.

"The remarkable history of reciprocal hybrids is directly opposed to Darwin's view that the functions of the two reproductive elements are essentially similar, for in some cases it is impossible to breed from a female of one species by a male of the second species, while the male of the first species readily fertilises the ovum of the second, and gives rise to offspring. Even when both ovaries are fertile the one is often much more so than the other."[27]

"If we take animals in which the sexes differ but little, and prevent them from following their own inclinations, and pair them without any reference to their own preferences, and continue this for a number of generations until we have produced a number of divergent races or breeds; if we then find that the males of those breeds differ more from each other than the females, we must conclude that there is behind the action of selection some more deeply-seated law which determines that the males shall, as a rule, be more modified than the females."[28]

The cause of the throwing back in mares to a good horse or to a bad one among ancestral links has never yet been, in any way, explained; nor has the fact that it is with mares as with bitches, that often the progeny are influenced by a former impregnation.[29]

There have been various cases of sows producing pigs with a distinct series of markings, the same as characterised the first litter, and Darwin refers to one case where this was a very pronounced chestnut tint.

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[27] "Degeneracy," p. 138.

[28] *Ibid.*, p. 218.

[29] Humfrey, p. 17.

The first breeding of a mare with a quagga affected the whole of the after-offspring. Mr. Darwin quotes several other cases of this same thing; notably a case reported by Mr. Jenner Weir, that occurred to his neighbour, Mr. Lethbridge, of Blackheath. A Cotswold white-faced ram, on a Suffolk ewe, has been known to have effect on the after-progeny.

V.

And why is it, with regard to canaries, that if two or three hens are paired with one cock, the first hen will invariably remain the favourite? Only when she is about to sit will he pay any attention to, or devote himself to either of the others, and will do no more for them than perhaps aid to feed their young; and yet it is, in fact, most definitely ascertained that from these later mothers almost invariably the best, strongest, and finest birds are got. Mr. Gould speaks precisely to this effect at page 81. Here, you see, that polygamy may have a very definite service to do for a species (under confinement, at all events) with regard, not only to the preservation of the species, but to the maintenance and improvement of it. How does this hold with regard to polygamous birds and animals in general? Do they, like the canaries, have the finest birds or progeny by the later mothers, or not? How about pheasants? How is it here with our ordinary fowl? How is it with regard to polygamous animals in general? How about the progeny of deer? Has it been observed that any such rule holds about the different progeny by different mothers? I should be glad to hear about these points from anyone who has observed and noted the phenomena. For it is clear that, if anything like this principle is established with regard to any extended variety of species, you have a very good reason in utility for polygamy—two good reasons, indeed: (1) the improvement of the species, and (2) the due proportion of the sexes to ensure this result—the finest and best specimens resulting from the union of males with second or later wives. And it has often occurred to me whether in collective mound-building you do not have, after all, a system of polygamy.

This is a vast subject with wide practical bearing, for if I am right—if I am in the least right—then it follows that in polygamy you have the assurance of the maintenance of high virility—breeding power in the male and the possibility of finer varieties, whereas under true pairing, especially for life, the chances are limited, and under polyandry, because of what I have laid down as the inevitable shrinking and lessening of the sexual organs in both sexes you have the tendency to exhaustion—the species not only will not increase at the necessary ratio, but it will more and more content itself with the mere fact of coupling or contact, caring less and less for the assurance of progeny and the cares associated with the feeding and bringing up of progeny. We see this in the cowbirds of America

—the *Molothrus* family—immense numbers of the eggs being lost. We see it in degree in the American cuckoos, which from building mere straws of nests lose a lot of the eggs too and drop them about and try them in other birds' nests; and, above all, we see it in our *Cuculus canorus,* or common cuckoo, which has advanced more definitely than either of these to the stage of studied and deliberate parasitism, by which it may manage for ages to maintain itself, immensely aided as it is by the wholly mysterious instinct in numbers of little birds, which leads them readily and assiduously to minister to its young. But for that fact, it is impossible that *Cuculus* could survive—a case, where, as I have elsewhere pointed out, a species survives and at least maintains itself, not by its own efforts and actions or its original instincts, which it has wholly abandoned, but chiefly by the most mysterious calling forth of separate and special and, in a way, most unnatural and *perverted* instincts in other species of birds. Here is precisely a case where the existence of a species is maintained by misdirecting instincts of other creatures, which work to the absolute injury of these creatures, alike to the individual and the species, and work only and absolutely to the benefit of the cuckoo species—supplying the very case—on the strength of which Mr. Darwin said that, when found, he would wholly surrender his theory; and clear it is, as clear as daylight, that Mr. Darwin never in the least mastered the facts about the cuckoo, not to say reached anything like a true theory of it, any more than Mr. Romanes did.

In birds that practice polyandry, as the cuckoo does, it will be found that there is a tendency to eleminate all the bolder distinguishing sexual secondary marks found in other birds, and more especially in the polygamous birds and all animals that fight for the females. In all the cuckoos we find in handbooks that, save for a trifle difference in size, the female is practically the same as the male, and any little distinction there may be vanishes with age, for what Dr. Bowdler Sharpe says of the great spotted cuckoo is really true of all the cuckoos :—"*Adult female.*—Similar to the male, but with a good deal of chestnut on the quills, which appears to vanish with age, so that there is at last no difference between the sexes beyond that the female is a little smaller."[30] And with the American cowbirds (order *Molothrus*) the female differs from the male only in being a shade lighter in colour. Polyandry thus seems to us, as we see it in Nature, to be an agency in so far working against the law laid down by Brooks in his "Heredity," and quoted a few pages back, to the effect that Nature has laid down a law beyond natural selection that the males shall, as a rule, be more modified than the females ; for here, so far as we can read it, the females seem to be more modified than the males. Man thus, indirectly as well as directly, works, and cannot help working, to disturb certain tendencies of Nature left to itself. Artificial selection is set up against natural selection in certain aspects, but really the effect of man's presence and effort indirectly and universally interjects new elements, new influences, working, modifying, and transforming—sometimes slowly, sometimes more quickly—the whole processes; and this is a matter that deserves far more attention than it has yet got; and, though Mr. Darwin did much, he diverted attention from this point rather than concentrated and directed it upon it. Unconscious artificial selection brings its own contribution, say, to the transformation of the American cuckoos into parasites, as more than one American naturalist has hinted.

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[30] "Allen's Handbook," ii., p. 22.

And with more than the cuckoos, with the cowbirds also. That admirable writer, Mr. W. H. Hudson, speaking of the *Molothrus bonariensis*, says:—

> "It continues in better condition than other species, not having been engaged in the exhausting process of rearing its own young, and, moreover, being gregarious and practising promiscuous sexual intercourse, must lay a much greater number of eggs than other species. *Hens that never become broody lay a great deal more than others. In wild districts where the parasitic instinct was found, and where birds building accessible nests are proportionately fewer, the instinct seems different from what it does in cultivated districts.* Parasitical eggs are not common in the desert, and even the most exposed nests are probably never overburdened with them. But in cultivated places, where their food abounds, the birds congregate in orchards and plantations in great numbers and avail them-

selves of all the nests, ill-concealed as they must always be in the clear, open-foliaged trees planted by man."[31]

This is all in favour of our thesis here: (1) As regards man's unconscious influence in the selection that, we hold, lies between natural and artificial selection; (2) as regards procreation as lowering conditions and shortening life; and (3) as to non-brooding of eggs tending to a greater laying of eggs; and (4) that loosening thus the habit even in the lower animals, and passing into abnormal relationships under changes effected by man, the finer traits are directly operated against even in the lower animals. This is confirmed by the report of Dr. Elliot Coues, who tells us:—

> "The cowbirds never mate; their most intimate relations are no sooner effected than forgotten; not even the decent restrictions of a seraglio are observed; it is a perfect community of free-lovers, who do as the original cynics did. The necessary courtship becomes in consequence a curiously mixed affair. During the period corresponding to the mating season of orderly birds, the patriarchs of the sorry crew mount the trees and fences, and posture and turn about and ruffle their feathers to look bigger than Nature made them, ... while the females, perched near by, without seeming enthusiastic, take it much as a matter of course, listening at times, it may be, but as likely preening their plumage, with other thoughts and an ulterior purpose. The performance over, a very little while afterwards the whole band goes trooping after food to the nearest cattle-yard or pasture."

Mr. Greene has in the *Pall Mall Magazine* for June, 1899, indicated very clearly some cases of purely artificial selection, when, for instance, he speaks of "ducks and geese as having the reputation, especially the latter, of being dull, stupid creatures, but far from it, as a rule they are very affectionate and faithful, not only to each other, but to their human friends. Both ducks and geese are naturally monogamous, but, corrupted by association with mankind, both the gander and the drake now permit themselves to have a plurality of wives, and some of the former have even adopted communistic views and act accordingly; but it was not always so with them."

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[31] "Argentine," pp. 77, 78.

But effects quite as decided resulting from man's efforts, though any intention of modification here is not in his mind, are to be traced over large areas of life, showing how man's "improvements" and the changes directly effected by him, modify as surely the determining conditions for whole hosts of creatures; and when man thus becomes, though without intention, the agent of such changes, you have an element over and above those recognised truly in natural selection alone, or in artificial selection alone; and till you have found a means of distinguishing and notifying these, you lack one of the organs, philosophically speaking, for scientific precision and progress. This is one of the somethings which lie, so to speak, between pure Darwinism and mixed Darwinism; and till you have recognised and defined it, a great gulf will subsist between the two, as also between natural selection *per se* and artificial selection *per se*. The scope of this something between the two, now that the world is not so wide as it once was, and but a few desert ranges that have not been surveyed and visited if not, in a sense, occupied by man, is probably far more extensive than either, and the phenomena it would present, were we in possession of the facts, would, in our idea, be far more enlightening than any with which we are as yet acquainted.

Mr. Darwin, as we have seen, came to hold that, when he found both sexes of birds so duskily coloured that it would be rash to assume agency of sexual selection, and no direct evidence can be advanced in proof that these colours serve as protective, then it was best to *attribute the result to direct action of the conditions of life.*[32]

From our point of view, here Mr. Darwin tries logically to separate certain causes in themselves and their interplay where, in fact, there is no *real* separation. In certain birds we know that conditions so directly act on the sexual organs that they are inseparable, as Haeckel has well put it, that the condition of the sexual organs in certain cases directly affects the secondary sexual characters and marks; that certain changes of food, as found by experiment in the case of frogs, affect the proportion of the sexes in the progeny; and that this fact soon comes to affect both the sexes alike in their habits and in their structure. Here we think especially of polyandrous birds, cuckoos and cowbirds of America, as finding in them scarce any outward distinction between male and female and a tendency to darkness or duskiness in colour, while assuredly we cannot in these cases find anything to justify us in the means of explanation suggested to us by Mr. Darwin above.

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[32] "Descent of Man," p. 491.

Sir Richard Burton has some very queer speculations about polygamy and related topics. I am fain not to complicate matters by going into questions related to those of human development; though, if I were to do so, some strange analogies between animal and human life would, on these lines, appear.

Of course, any such idea as this has to be taken in connection with, or never separated from, many co-operating joint influences, more especially among such animals as deer, which are really, in a country like ours, under artificial treatment by men so far, and are not, in any sense, really free and wild, as they may seem. Among these are feeding, proportion of the sexes, and so on. No doubt, one very powerful tendency is to constant degeneracy, through the killing of all the finest stags to gratify what is called the "pride of sport." This alone may have very marked and deteriorating effects, beyond what any mere general observer could perceive. Mr. Darwin certainly noted these facts, and based a very telling contrast on it. He writes :—

"The Incas followed exactly the reverse system of that which our Scottish sportsmen are accused of following, namely, of steadily killing the finest stags, and thus causing the whole race to degenerate."[33]

But men, if true sportsmen, are doing in part the same, wherever they proceed to shoot game, and especially big game; and where now, in the wide world, do they not go for this purpose?—so that the effect upon species, not alone on the species directly affected, but on others related to, if not in so far dependent on them, needs, much needs, to be traced and estimated; and if it is difficult to estimate this in the case of deer constantly looked after, how much more difficult in the case of such creatures, and wild creatures, elsewhere? The instinct for sport in the true sense—sport which leads men far a-field—is thus a yet unestimated factor in selection, which is neither natural nor artificial, strictly speaking.

Mr. Richard Jefferies, in his "Red Deer," has this passage about the habits of the stag when shedding his horns:—

"He separates himself from his companion stags, and keeps alone at this moment. It is possible that he may drag brambles or branches over the dropped antlers, if they chance to grow at hand; for it is remarkable that few horns are found compared to the number that must be shed, and those that are found are more often single horns than pairs."[34]

And with regard to the protective character of the horns, it is suggestive that the stags go apart, like the females, at the very moment they are most defenceless; whereas numbers and union would here certainly be strength—here as in so many other cases.

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[33] "Domestication," p. 208.

[34] "Red Deer," p. 65.

Scrope, in his day (1838) could give no account of what became of the shed horns, so few being found compared with those shed, though they are perquisites of the keepers. "Hinds have been seen to eat them; one will consume a part, and, when she drops it, it will be taken up and gnawed by others. The late Duke of Athole, indeed, once found a dead hind which had been choked by a part of a horn sticking in its throat. It is not, however, credible that all those which are missing are disposed of in this way; they rather seem to be thus eaten from wantonness and caprice; and I am not able to account satisfactorily for their disappearance."[35]

Nor has later observation and experience resulted in anything much more definite. Here is a note from Mr. Macpherson's book on "Red Deer":—

"The immense quantities of bones deer will eat is proved by Mr. Williamson's statement to Mr. Harvie-Brown, that in a few months they had completely eaten the bones of a horse in the Hebrides, and Mr. Harvie-Brown remarks that this great appetite displayed may be accounted for by the total absence of bone-producing elements in the geology of the Hebrides."[36]

Yet the deer of the Long Island have fine heads, with more points than mainland deer.

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[35] "Red Deer," p. 6.

[36] *Ibid.*, p. 34.

Major-General Crealock has some very suggestive speculations suggested by these facts. He finds that a certain proportion of lime in the food is demanded by the deer, and more especially by the hinds at certain times, as well as salt. This latter should be artificially supplied by throwing down pieces of rock salt, which they will lick precisely as horses do in their mangers. As to the supply of lime, the General recommends the putting of sheep for a time on portions of the forest and shifting them from place to place till it has been pretty well gone over, as sheep deposit more lime than deer do. In truth, this whole question of salt in its influence on birds and other creatures wants a lengthy and exhaustive treatment, which in another work we shall try to give it before very long. It is a notorious fact that shepherds in Switzerland and elsewhere actually spread salt or throw down piecs of rock salt in the pastures for the sheep, as Major-General Crealock here recommends for deer. The lack of salt in these cases develops in the sheep tameness and utter fearlessness of the human figure. At certain times this extra salt is essential; and, whatever theory may be held about it, there can be no doubt that nearly all creatures desire salt, and that the amount which is supplied to them by natural processes, not to speak of creatures deprived of it in confinement, is often found insufficient. It seems to me to be a very powerful agent in promoting migration with many classes of animals, and this is the more assertive when we find domesticated animals when in want of it actually run up, putting their heads close even to a stranger, as tame deer in Greenwich Park, &c., will do for bread, &c., &c. And on the ground of the imperative demand for lime Major-General Crealock accounts for the disappearance of the vast numbers of horns that must be shed. They are eaten for the lime in them by the hinds. Major-General Crealock writes :—

> "It is a curious proof of the lime argument being accurate, and that nature requires lime for these animals, that you rarely find any shed horns on a forest occupied by hinds, though you may sometimes stumble across a scrag end of a horn all gnawed and eaten by these ladies ; but it is worthy of remark that in stag ground, where a hind is rarely seen, you will often find shed horns, and ungnawed and perfect, which goes to prove that stags do not eat shed horns, though some believe that they do so as well as hinds. All I can say is that I never saw it, while I have seen hinds doing it constantly, and have now in my possession several horns half eaten by them. Nearly all the evidence I have been able to collect on the point goes to show that stags do not eat horns ; the nearest thing to it I can quote was seeing two young staggies in 1890 eating some bones at Glendole Forest. I have seen hinds in N. Harris actually gnawing the points of a stag's horn on the living stag, and in one instance I killed the stag and found his horns all gnawed and scored by their teeth. I have the horn now, and the stalkers of that forest tell me it is of common occurrence there, though I have never seen it done on the mainland of Scotland, and none of the stalkers whom I have questioned have ever seen it. In any case, whether the lime theory be accurate or not, there can be no sort of question that heavy deer and fine heads are the result of lime, good nourishing food, and efficient shelter against the bitter storms and cold of winter. The heaviest deer and the finest heads the wide world over are the so-called wood deer ; the huge brutes we see in the pine forests of America, in Germany, Hungary, in Asia Minor, and the Himalayan range are all, properly speaking, wood deer, beasts

that rarely leave the covert and protection of the forests which afford them good food and shelter from sun and cold."[37]

Whitaker, on the other hand, declares that "both bucks and does will pick and chew shed horns. I have seen them also chewing old rib-bones of beef."[38]

Another very peculiar point is that with certain of the Indian deer, if indeed it does not apply to all Indian deer, the bucks, or certain of the bucks, do not shed their horns annually.[39]

Mr. Millais tells that the dressing of the Warnham Park grass with bone-dust every alternate year has added 70 per cent. to the nutrimental qualities of the grass, accounting for the immense improvement since Mr. F. M. Lucas's plan was tried, a four-year-old Warnham stag being better than an adult animal in most other English parks.

VI.

(1) On the question of whether or not stags eat their own or other horns, an important point would of course be the fact whether or not there was a due proportion of lime in the soil or only a defect of it. This is a matter that wants closer investigation with regard to the varying habit of the deer in this particular than it has yet got. There is no doubt that the does do eat them.

(2) Whether the fact that in many cases antlers have been found buried or half buried in the soil or

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[37] Crealock, p. 10.

[38] *Ibid.*, p. 10.

[39] Forsyth's "Highlands of Central India."

covered up with branches, &c., as Mr. R. Jefferies particularly dwelt on, does not point to some peculiar instinct in the buck as regards possible effects in the females if the eating of horns should be overdone.

(3) Whether high feeding and in-breeding have any influence on the limiting of the time during which the buck is procreatively potent, whether it definitely hastens impotency or does not, and also its influence on longevity.

(4) Whether the castrated stags have a tendency to longer life than the procreating bucks, and to what extent. Observations in this direction are rendered the more rare and the more difficult because the haviers are usually killed for venison within the year. But the results, were they procurable, would be of great scientific value in view of certain of the principles laid down by Professor Ray Lankester in "Longevity."

(5) What is the rule—the result of experience—as regards the transport of deer of our breeds to other climates, more especially tropical climates? Do they or would they in India gradually or more or less suddenly cease to shed their horns annually, as most of the Indian deer have either ceased to do or have never done?

(6) What are the reasons why the Indian deer do not, like our own, shed their horns annually; and has this any effect, direct or traceable, on the generative organs; and how do they stand for age attained?

(7) Are there any systematic attempts whatever at estimating the loss to herds of deer by degeneration in course of years through the shooting year by year of the finest stags?

(8) Have exhaustive experiments been tried on the effects of strewing salt, preferably rock-salt, liberally on the feeding-grounds, especially at certain seasons, as recommended by Mr. Millais and Major-General Crealock, and also on the effects of supplying additional lime in any form in the same way?

(9) As there cannot possibly be a doubt that not only old and sexually outworn females, but does in young and giving milk, have had horns, has any peculiar circumstance beyond those noted been detected which could account for the departure from general character or the reversion to earlier type? Mr. Darwin's explanations are inefficient and do not include or recognise the most important facts.

(10) If a useful frontal instrument for defence or offence once common to both sexes becomes proper to only one of them, clearly if it is justified under sexual selection, some violence is done to natural selection, since, in certain circumstances, such an organ of defence must be needed by the females, and the more needed when separated by long periods from the males and during the time of the suckling and rearing of the young. Can it be said, then, that natural selection will justify the *aborting* of useful frontal instruments of defence in those which most need them, and that in view of the safety and pre-

servation, not to say increase of the race? The sexual selection idea here *may* apply to increased and improved horn growth in the males, but it cannot surely apply to the aborting of useful frontal implements in the females—a point, moreover, which has confirmation in the fact that in some breeds of deer the females are still horned. Sexual selection is held to be a complement or aid to natural selection; here we assert the one is diametrically opposed to the other, and they cannot, on the facts, be reconciled.

But if, on the other hand, growth of horns is largely determined by good winter feed, such as the creatures hardly could have continuously enjoyed in wild nature (as Mr. Tom Speedy says, "no such horns could be found in wild nature as are found under artificial or semi-artificial park tendance"), how account for these splendid antlered peat-moss deer? The question, therefore, arises in how far the development of fine heads in deer may be due not to sexual selection or to strictly natural selection, but to that wide field of influence as yet so largely overlooked and indiscriminate—the field of unconscious and unintentional selection, due to the changes and influences brought about indirectly by man's operations upon nature in changing wild to cultivated and partly cultivated?

Mr. Macpherson, for example, in our idea rather short-sightedly, takes Mr. Lydekker to task for saying that the Westmoreland deer are not truly wild because at least some of their number receive a little hay in winter, and reinforces his position by adding that the deer of the Scottish forests received the same aid in hard times. It is clear they could not receive that aid in the purely wild state; therefore Mr. Lydekker is right; they are not *purely* wild in exactly the degree to which they receive it, and this the more that growth of horns, &c., is so closely dependent upon such aids; and Mr. Macpherson himself fully admits this a few pages further on: "The condition of deer is largely governed by the character of the food supply which they command."[40]

"The little hay in winter,"[41] which Mr. Macpherson regards as nothing, is representative and symbolical to our mind of a host of things which cannot be so definitely noted, and yet are powerful, though man does not precisely intend it, to change the whole field in which animals live and move.

If the polygamous condition of the pheasant in the half-domesticated way in which it is kept in this country is to be attributed largely, if not wholly, to the fact that in shooting far more cocks are killed than hens, and that the cocks even sometimes take to the sitting on eggs, why should it be supposed that deer, when far more stags are shot than fawns, have not been influenced and modified in the same way? They must be influenced and modified so far in the same way, and wherever Nature's proportion of sexes is interfered with you have modifications in the most vital points—sexual habits are changed—and in so far as they are so the subjects are not truly wild.

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[40] "Red Deer," Fur and Feather Series, p. 23.
[41] *Ibid.*, p. 28.

Careful observations and notes on the above points would, I am fain to think, do much to enable us to draw from the life-history of the deer conclusions of the utmost value, not only as regards certain principles laid down by Professor Ray Lankester in "Longevity," but materially help to the solution of certain problems raised elsewhere.

Postscript.—To me it is one of the most extraordinary omissions in the work of any great author that Mr. Darwin paid comparatively little attention to deer. A dozen lines would exhaust all that he has to say of them in "Origin of Species" or "Domestication," though the peculiar phenomena they present were entirely of the sort to supply him with suggestions in many directions.

VII.

Professor Cossar Ewart's experiments in crosses between horses and zebras, so far as we yet have them, *seem* to establish one general dictum of Mr. Darwin's in that the stripe markings appear to revert to earlier types, while in certain practical matters they are disappointing, and certainly so in the results of inoculation by the tsetse organism, where the hybrids, while showing somewhat more resistance than horses, all died in about eight weeks. Professor Ewart has one positive result: he declares that the markings more accord with those of the Somali zebra than any other, and this he regards as the oldest in its colour pattern of all the genus.

So far as telegony is concerned, the results were certainly not decisive, for in "Mulatto's" second crossing with a grey Arab stallion, "Benazrah," though the foal was certainly striped, the stripes were not like those of "Mulatto's" first mate, "nor, on the other hand, did they entirely resemble those stripes often obscurely visible in ordinary foals." So that, though the general fact of telegony is hereby established, no light whatever is thrown on modifying circumstances — backthrows, it may be, of very complex kind; nor on the tendency here certainly suggested to throw still further back in the most amazing manner on very early types. Perhaps Professor Ewart's further experiments may throw some light on this puzzling and obscure point—so important in any effort at mastering the laws at work in hybridity. One important point I forget—a point I ought not to have forgotten, as no doubt Professor Ewart tells it—but were these two foals of one sex or not? Reversion is all very well as a phrase, but here the whole thing is, as the Yankees say, too "mixed" for being dismissed with one phrase.

Anyhow, so far as we've got, as Artemus would put it, the case is not in other respects so clearly in favour of Darwinism as at first sight it appears. For here there is combined a most remarkable reversion to what is to us as yet an indefinite earlier type, with variations such as we cannot at all see to be in any respect *useful*, unless, indeed, Professor Cossar Ewart should by and by be able to tell us by evidence of indisputable facts that here we have a confirmation of Dr. Brooks's leading idea of a law beyond natural selection which provides that hybrid males should also be more modified than females. If then any token of reversion might be shown to come under this great law, we should perceive behind the seeming inconsistency of the markings a certain unity of aim and result in nature's methods in this kind to which certainly Mr. Darwin never fully awakened.

VIII.

We must now return on a point formerly raised. The notion that deer's antlers of larger size than ever occurred in nature have been produced under semi-domestication, where aid could be given to the feeding in winter, is dissipated by geological finds. Dr. H. Woodward has described in the *Geological Magazine* for February, 1898, the restored horns of a red-deer (*Cervus elephas*) found in a tufaceous deposit about 1780, and described then by the Rev. Robert Barber, D.D., in a letter to John Jebb, Esq., M.D., F.R.S., and published in the *Transactions of the Royal Society* for 1785 (vol. lxxv., p. 353). They were of great size, "resembling," says Dr. Woodward, "the great American wapiti (*Cervus canadensis*) in stoutness and length of beam"; and he quotes Dr.

Barber, who says:—" Though the horns [antlers] are so much larger than those of any stag I have ever seen, yet from the sutures on the skull appearing very distinct on it, one would suppose that it is not the head of a very old animal. . . The horns [antlers] are of that species which park-keepers in this part of the country call 'throstle-nest horns,' from the peculiar formation of the upper part of them, which is branched out into a number of short [tines or] antlers, which form a hollow about large enough to contain a thrush's nest. . .

" The river Larkell runs down the valley, and part of it falls into a quarry where these horns [antlers] were found, the water of which has not the property of incrusting any bodies it passes through. It is, therefore, probable that the animal to which these horns [antlers] belonged was washed into the place where they were found, at the time of some of those convulsions which contributed to raise this part of the island out of the sea.'"

The following are the measurements of the antlers, since they have been mounted and repaired in the Gallery:—

			Ft.	ins.
Width of the "nests"	...	...	3	9
Length of right antler	...	...	4	0
" left "	...	...	3	8
" brow tine	...	...	0	11
" 2nd "	...	...	1	0
" 3rd "	...	...	1	1

	Ft.	ins.
Girth of pedicle	0	7½
,, above the burr	0	9¾
,, of 1st tine	0	9¼
,, „ 2nd „	0	6½
,, „ 3rd „	0	6¼

The *Cervus giganteus*, whose remains have been largely found in Ireland, in the Isle of Man, and in parts of England, frequently measures 9 feet across the antlers, the weight of which would be as much as from 80 to 90 lbs. This, though often called the "Irish elk," has nothing whatever in common with the elk, but it is a true deer in every respect, and no doubt, says a good authority, "in throwing out, in the course of a few months, this mighty mass of osseous matter, followed the same rules as those which govern the horn-growth of other deer to-day." When one thinks of the production of blood, &c., necessary to the forming of such horns, we well may wonder how the animal could undergo such a process year by year. We read :—

"The fossil remains of deer, which have been plentifully found in this country and the north of India, indicate that when unmolested by man and in a wild state, they attained a far greater size, and probably age, than at the present day. The *Cervus giganteus* found in the Isle of Man, according to Mr. Kermode, must have been about 18 hands, or 6 feet, high at the shoulder."[42]

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[42] *Geol. Magazine*, 1898, p. 118.

Again, we have this very significant and suggestive passage :—

> "There is good evidence for the conclusion that after the great deer had spread into Ireland, and probably long before its extinction in this country or in Western Europe, Ireland must have become isolated from England, and during a long succeeding period the *Cervus giganteus* lived and flourished in that island, and was neither exterminated there by prehistoric man nor by any of the carnivora, but by a great and gradual change which took place in the climate of that country. This change, in which Scotland and a part at least of England also partook, was an increase in cold and a settled humidity of climate, tending to a great growth of peat, which in time filled up the former extensive fresh-water lakes once so abundant, and injuriously affected the forest-growth over large areas of the country. With this change the great Irish deer died out ; but its remains show that it was living there before the growth of sphagnum or bog-moss had taken place, for they rest in the shell marl beneath the peat. This shell marl is really composed of the accumulated deposit of dead and decomposed shells of fresh-water *Unio*, *Anodon*, and *Limnea*, so that it represents a long and tranquil period of time during which conditions were favourable to forest-growth, and consequently to the deer and the other denizens of the woods and the waters."[43]

This is all in the direction of establishing the generalisation of Sir R. Owen, who long ago pointed out that the large mammals were always the first to suffer from floods or from droughts, events which happen most frequently within the tropics, but which may occasionally in any other country.

The point of our argument here derived from these facts, and from other facts presented in earlier parts of this section, is (1) that the notion of superior feeding provided by man in winter, while it may meet certain specific conditions due to his own semi-domestication of these creatures, is not absolute as to alone or in itself favouring great antler growth; and (2) that, since it is so, or seems to be so, some deprivation of element in grass, &c., must arise (as, in fact, practical sheep-rearers and breeders have found it to be so with sheep through too long restriction to one feeding ground, too little change from pasture to pasture, and too little intermixture or crossing), simply through (*a*) restriction of area, and (*b*) through the killing off of the finer stags and reducing the number of males of necessity, thus making the race more and more polygamous. Under conditions of pure nature nobler stags and heads were produced, as geology proves abundantly; and two most important questions are here raised—(1) as to the point or date at which the females in the various species ceased to bear horns; and (2) whether in the early periods when they did so the female was as splendidly antlered as the male. Evidence on these points, hard, hard to get, would be most helpful.

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[43] *Geological Magazine*, March, 1898, reviewing Mr. J. G. Millais's book on deer.

General Crealock speaks about putting sheep temporarily on areas run over by deer on account of their droppings supplying more abundantly to the soil and to the grass growth, &c., what the deer take out and apparently do not return by their droppings in anything like the measure sheep do, which just suggests the question whether the great horn growth absorbs, as very probably it does, much lime which is translated through the blood into these very antlers, and, of course, not returned to the earth. In perfectly wild life herds of deer are not segregated, watched over, and, as far as may be, kept inviolate from intrusion; other creatures run freely over the same ground, feed and do all other functions of nature; and so here a balance is preserved which must *in measure* be lost whenever you lay out a park, or even a deer forest, however extensive, implied in the very fact that in some degree it is artificially looked after and kept as such, and certain creatures, if they exist there, are hunted and killed off, as deemed to be inimical to deer, or as consuming too much of their food, &c. But here we have, in addition to proof of other things, the most decisive proof of this—that antler growth in deer, a growth that has come to be so distinctly connected with and bearing on the growth and health, &c., of the sexual organs, is absolutely dependent on conditions, on the food, and certain specific elements in the food, &c., and perhaps on neighbourhood with creatures of other species; for as the grand old Puritan divine said: "One thing is set over against another, and there is nothing single or separate."

PART IV.

MR. DARWIN AND "SOME CHANGED CONDITIONS."

I.

WE now return on the general subject, with a leading from Mr. Darwin. He says:—

"Changed conditions of life produce some effect on the fertility of our most thoroughly domesticated animals, in the same manner, though in a less degree, as with captive wild animals."[1]

Mr. Darwin here put "unnatural" along with, and in front of, "changed conditions," but I choose to leave the first word out, for a good reason. Mr. Darwin had just shown that *a very slight* degree of difference in the strictness of the confinement would effect, in many cases, all that he had set down. Such a very slight change cannot be, in the strict sense, called unnatural—not, at all events, in the sense that the reader would at least infer. And, in fact, Mr. Darwin, some dozen pages before, had himself written so that his use of the word "unnatural" here is, *by himself*, ruled out. He had said:—"A *slight* change in the treatment of animals sometimes makes a *great* difference in their fertility."[2] He had just written, too:—"In Europe close confinement has a marked effect on the fertility of the fowl; it has been found, in France, that with fowls allowed considerable freedom only 20 per cent. failed, and in close confinement sixty out of the hundred were not hatched."

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[1] "Domestication," ii., p. 162.

[2] *Ibid.*, ii., p. 149.

Now, what does this imply and must imply—that the creature in close confinement, if not positively ill-treated, is relieved wholly, or almost wholly, of the trouble of finding its own food. In the very measure that it gets freedom, it takes up, and must take up, the task of in so far finding food, or in so far, at least, supplementing its artificially-given supply. There is no escape from this. Fowls in a small run cannot scratch up food, as on soft, fresh earth, on dry, hard, and oft-trod earth; fowls free on a farm or in a farm-yard can so do, by the very fact of being there. And yet Mr. Darwin writes as though wholly unconscious of what he had already written:—"With hardly an exception, our domesticated animals, which have long been habituated to a regular and copious supply of food, without the labour of searching for it, are more fertile than the corresponding wild animals."[3]

I deny it, and have found this to be false in my own experience. The fertility of fowls, if taken over a certain period, is pretty exactly in the degree in which they can naturally exercise their natural tendency to scratch up earth and find food for themselves.

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[3] "Domestication," ii., p. 111.

Why, even in a little handbook of poultry (Goulding's) we read:—

> "It is a bad plan to throw down a lot of food to fowls to let them stuff their crops, and then allow them so little room that they can scarcely turn themselves. Superabundance of internal fat may be the result, but good firm meat, strong muscle, or a healthy constitution, is not got by this treatment. Give ample feeding room, and *let the food be scattered, so that the fowls will have to walk about to pick it up.* Where you have not a proper space of green for them, it is a good plan to hang up cabbage stalks and lettuce leaves for them to pull at as they wish. The water pan or fountain should be cleaned out every day, and a constant supply of clean water given. . . . Animal food is very necessary for poultry, and where they have not a plentiful supply of worms, grubs, &c., a little cooked meat cut up into small pieces should be given to them. Fowls should have a mixture of food, hard and green together, and as much exercise and movement as it is possible for them to have in the circumstances."

Here the practical man is with me and wants to work on my principle as regards poultry. He wishes them to have as much exercise as they can get and to be made to search for their food as much as the nature of their run will at all allow; and he does not agree with Mr. Darwin that "domesticated animals, which have long been habituated to a regular and copious supply of food, *without the trouble of searching for it,* are more fertile than the corresponding wild animals."

Even in the case of "Answers to Correspondents" in such papers as *Poultry, The Feathered World, &c.,* nothing is of more constant occurrence than the reply that conditions—food, space of run, state of earth in run, raised up or level with the outside, &c.,—are not accurately or fully given. Mr. E. Cobb, F.Z.S., in *The Feathered World*, is very particular on this point, and scarcely a number appears that does not have the warning against "throwing down food so that the birds can go to it whenever they like." He, at least, is not in favour of Mr. Darwin's constant (regular) and copious supply!

The healthiest and most productive pigs are not those which are kept for ever in a stye, but those on a farm which are at least now and then turned out into the fields and have their run about picking up what they can. Thus they have exercise, a chance of cleaning themselves, and of breathing pure air; as Mr. Darwin, just quoted, has told us about fowls.

I recall a fine saying of Lessing, and would apply it here; it is not the finding, but the search that profits. With the domestic hen, as with the stallions, both here and in India, it is needful that in so far they work for their living; it is in the working for this that it all lies. It is the law of the world. But while Mr. Darwin, with the one hand, is trying to prove that it is indeed "regular and copious supply" of food provided without effort or search, that is, as he holds, the *conditio sine qua non* of fertility; with the other he is supporting my view *in toto*, and, at the same time absolutely refuting all he had written in his first three books, that natural selection was practically independent of external conditions.

Pretty much the same applies to men and women. Certain of Lord Rosebery's words come to me with power, and are apt, most apt, to my present purpose :—

> "However wealthy you may be, you have only one body to dress or decorate, and you can only ride one horse at a time unless you are practised in the games of the circus ; and, in fine, reason it out as you may, you will find that the mere gratification of the individual cannot lead to any further expenditure ; and even in the gratification of luxury there are counterbalancing considerations.
>
> "The man who lives less well than another is apt to enjoy much more vigorous health, and, therefore, in most requisites, life is not, perhaps, as between rich and poor, so unequal as it seems to be."

Life is just as unequal as it seems to be, and is. Though some degree of health may be assured by the fact of necessity for picking up, as with the fowls, still all depends upon the food, or some food, being there for the picking up ; and with the poor sometimes it isn't there—neither in sufficient quantity nor quality, while with Lord Rosebery he has of necessity no healthy labour of picking up ; and, it may be, both suffer in the progeny, and life of rich and poor, thus unequal as to its means of gratification of appetites, may still be equal in certain of the ends attained. Some poor creatures, sober and well-intentioned, as coroners' courts prove, alas, have died of starvation ; and some wealthy persons have died, as other courts have proved, of surfeit—there Lord Rosebery is right —right as a trivet ; the end the same, though causes as different as starvation and surfeit. The poor man may meet with some good angel, and be saved from death, but never recover completely from the effects of the starvation; the rich man may move about with his chronic dyspepsia and misery, due to too much and too many; and there again the end is the same, though the means and the causes are so very, very *different.*

Thoreau, whom Lord Rosebery, in the face of R. L. Stevenson's own clear large-type printed words, denied had in the least influenced Stevenson in style, or in anything else, has a few words that will go far here when he says that the serious business of the early man has become the sport or pastime of the "civilised." They all try to make it appear as if they did something for their living—to find or to earn the food and good things they eat—they hunt, they go racing, they run about with their blood-horses—not always dark horses in loose boxes either—which they make like unto themselves and spoil as they go by only giving them pretended and artificial exercise instead of common healthy work-exercise; and then they think it is all right. But certain muscles remain in abeyance, and it affects so much, so much else. And Lord Rosebery, poor, poor man, finds his wealth so great a burden and finds the poor man so rich in his poverty, though, to my mind, it is exactly this, as the Great Eastern railway porter said to the gentleman whose luggage he was carrying, on seeing a fellow lying drunk in a corner, "Why, sir, that's enough—shows how ill the world's goods is diwided; he's got too much o' what I ain't got enough on." The poor man lacks some of the wealth Lord Rosebery has gone hunting for and got, and now says it is not all so fine and bright as he thought it would be, and Lord Rosebery wants some of the downright hard work the poor man has not so much gone a hunting for and yet has got.

II.

As I re-read Mr. Darwin's " Domestication " I made this note: Whether in confinement the sterility of elephants is not more due to want of sufficient exercise than any other cause. Mr. Crawford informed Mr. Darwin that the fact of elephants breeding in Ava in domestication must, he firmly believed, be attributed solely to the females being allowed to roam the forests with some degree of freedom,[4] that is, seeking for and finding their food in a purely natural way.

Mr. Darwin himself says that the elephant which has been long domesticated in India does not breed. " Elephants are kept in large numbers in their Indian home, live to old age, and are vigorous enough for the severest labour ; yet, with one or two exceptions, they have never been known even to couple, though both males and females have their proper periodical courses." And yet Mr. Darwin would fain have us believe that domesticated animals " which have long had a regular and copious supply of food without the labour of searching for it are more fertile than the corresponding wild animals." You may work an animal as hard as you will in harness of one kind and another, and yet that animal's head, neck, &c., never once be moved in the same way that it would running free, seeking its food in a natural way—a thing I have often thought of in looking on beautiful animals spoiled by the bearing rein in London streets and the high-stepping deformity. I wonder if Lord Rosebery sees anything there like what I see?

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[4] " Domestication," ii. p. 150.

No; Mr. Darwin is all wrong in his later work as in his earlier. As Haeckel says: "Certain influences act upon the male organs of propagation only, and affect the structure of the male descendants, and in like manner other influences act upon the female organs of propagation and manifest their effects only on the change of structure of the female descendants. This remarkable phenomenon is still very obscure, and has not as yet been properly investigated."[5]

The exercise of the muscles and other organs dependent for development on the natural finding of food is kept in close, absolute, and most inseparable relation with the healthy and normal development of the sexual organs, and respond with the most absolute precision to the exercise or non-exercise of these.

The more impossible it is for you in confinement to give the animal a chance of pursuing occasionally its natural habit of hunting and searching for, or of capturing its food, the more unlikely it is for you to gain your desire in that creature's breeding successfully; and never, I think, in all scientific literature have I met with a statement more thoroughly empty, astray from the facts, and preposterous than Mr. Darwin's oracular: "With hardly an exception our domesticated animals which have long been habituated to a regular and copious supply of food without the labour of searching for it are more fertile than the wild animals."

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[5] "History of Creation," p. 230.

There is no more highly and delicately fed creature then the race-horse — no more carefully tended domestic animals than certain stag-hounds, no more prized animal or one that more enjoyed a regular and copious supply of food than certain otter-hounds; no one of the dog tribe has been more thoroughly domesticated than the St. Bernard, or been made more nobly useful to man; yet the first does not breed so freely as to meet demands—hence, very fancy prices, two are almost extinct in their pure breeding, and the St. Bernard is going, going fast. The list might be lengthened vastly over nearly the whole area of domesticated creatures; but these are enough to show that it is not true in any approachable sense, not to speak of absolute and unqualified sense, that "with hardly an exception our domesticated animals which have long been habituated to a regular and copious supply of food *without the labour of searching for it*, are more fertile than the wild animals."

Fertile! cats and dogs, in so far free and natural conditions, are fertile, carnivorous mammals, see you? Fertile cats truly are; but the reason is that, do what you will, you cannot, unless you chain them up, keep them from roving and hunting after their own natural style; and in degree the same thing applies to dogs, and the reason they are so fertile simply lies in this. In the mass of cases you cannot deprive them of some indulgence in their natural hunt for food, &c., even in the most unpromising conditions, for, as Mr. Darwin at one place shows, the number of old maids in a locality determines the growth of clover: they keep cats, cats kill field-mice which destroy the nests and combs of the humble-bees which fertilise the clover, so that if there are many old maids who keep cats there will be good crops of clover!

And with regard to the breeding of certain very fine and high-classed birds of various kinds, Mr. Darwin's dictum is not only incorrect, but in application would be ruinous. At the very extensive and beautifully fitted up aviaries of Mr. Christmas at Silwood, Tulse Hill, everything is done to enable the birds to have natural exercise, abundance of fresh air, thorough bathing, &c. But the one thing guarded against is exactly what Mr. Darwin says is a *sine qua non*. The regular and *copious* supply of food, without the labour of searching for it, is precisely what is studiously denied. No warning is more impressed there on visitors than that "*copious* supply of food" (which spells overfeeding) is to be sternly avoided as leading to diseases of digestive organs, blindness, &c. So that Mr. Pelling, Mr. Christmas's deputy, is reported to have said :—

"Try to find out how much your birds can eat when given but one meal in the day, and that discovered, just divide that quantity into halves, giving one portion early in the morning and the other in the afternoon. Never leave food about; only give such a quantity as the birds will eat there and then with avidity."

This is given as one of the secrets of the "hard condition" which marks Mr. Christmas's homer pigeons,[6] and clearly these gentlemen do not believe in Mr. Darwin's "regular and copious *supply*," either.

Everyone who studies poultry or fancy bird breeding of any kind knows well that fatness is almost as unfavourable for laying eggs as is old age itself. If you fatten by intention for the table, you will not have many eggs. Here, as in the language of Artemus Ward, the witty, "you pays your money and you takes your choice."

Mr. Darwin notes that "while carnivorus birds are extremely sterile, carnivorous mammals are *moderately* fertile." What is the reason? You cannot, do what you may, launch an eagle in the air, with a string tied to its leg, to drag it back again at your own sweet will, in order to give *it* a bit of exercise in catching its prey naturally; the most natural exercise of the bird you cannot indulge him in—not in the least—and it is the want of exercise of this kind, to keep the hollow air-bones right and the blood truly circulating, that dooms him to sterility—whereas, up to a certain point you can let the mammal have some degree of natural exercise. With carnivorous mammals you *can* vary their diet, and you *can* do something more to give them exercise. You can introduce live food to them, which, like domestic cats, they can make a feint of playing with and catching if they will. Flight here is what you cannot give the eagle: you can contrive such means as to make a lion or a tiger run about, back, and around again, and, by introducing live creatures, let him now and then catch his prey and kill it; and that you can do this, and cannot do it with the eagle so as in flight to let him catch his prey, is explanation of the fact that "carnivorous birds are extremely sterile, while most carnivorous mammals are moderately fertile."

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[6] Mr. S. H. Lewer, in the *Feathered World*, July 14th, 1899.

Mr. Darwin writes:—

> "Strange as the fact may appear, many animals under confinement unite with distinct species and produce hybrids quite as freely as, or even more freely than, with their own species. On enquiring from Dr. Falconer and others, it appears that the tiger when confined in India does not breed (even moderately), though Mr. Bartlett is quoted to the effect that the lion breeds more frequently and brings forth more at a birth than any other species of the family. Mr. Bartlett added that it was 'remarkable that lions bred more freely in travelling collections than in the Zoological Gardens'; 'probably' he said, 'the constant excitement and moving from place to place,

or change of air, may have considerable influence in the matter.' "[7]

Which most distinctly is our idea of it, and the facts are solely in harmony with our principles.

But there are large areas in which Mr. Darwin's rule will not apply. Under the heading of "Sterility from changed conditions" he tells about domesticated squirrels, the common hare (which, by the bye, has crossed with the rabbit, though it will not breed with its own kind), monkeys rarely breed when confined in their own country; parrots never either there or in our own country. Change of climate cannot cause this, nor change of food (ostriches, ducks, and other animals which must undergo change of food breed quite freely).

Now, it is truly a most extraordinary thing, as I have found in my own long experience of birds in a night aviary, with free day flight in a considerable sized room, that the most valuable of foods for a change are the earthy roots of certain plants of which they are fond—plantain, groundsel, lettuce, &c., &c. If a whole plant, root and all, be put down—along with much else, my birds will go for the still moist earthy root first, and will peck and enjoy it for a long time. I put flower-pots full of fresh earth in the room, with a pretence of flowers in them, and oh, the joy they have in that earth is wonderful, and the seeds softened by lying in it, sweet, sweet to them as they find them by searching. Here, so far, they are with nature. Here they find, in confinement, something by, as it were, searching for it. I do my best to bring nature to the birds, and I am rewarded by fruitful breeding, robust health and creatures that enjoy their lives and—love me. Linnets, bullfinches, goldfinches, siskins, canaries—one and all—the first named, I think, the most affectionate and familiar and strong-brained of them all. The vast numbers of birds kept in cages cannot be so treated, because they are always in cages—to fly, to choose food, within as large limits as can be allowed in confinement. There is the secret; if you love birds and have not yet tried it, be persuaded, if you can, to give it a trial, you will have your reward. Canaries in this respect are yet as true to nature as linnets and other finches—and in some cases would hide up a root and fetch it out again, and find no end of pleasure and satisfaction in it, even after it seemed to me quite dry and sapless and gone to mere threads.

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[7] "'Domestication," ii., p. 151.

Mr. Greene writes in *Pall Mall Magazine*, June, 1899, "As most breeders of canaries know to their cost, the parental instinct is by no means a strong point with many of these birds which neglect their young in the most hard-hearted fashion."

I have kept and bred most kinds of canaries and birds with which they cross. My birds were free to fly about the room and free to mate and had free choice of various food, and only in one case had I ever any difficulty, through lack of parental feeling, and in this case I took the female from the young and caged her, and the young were fed by another canary hen, who had three young ones of her own, just able to fly, to look after. Did Mr. Greene find any difference in this respect after the change from caging to freedom? And might I venture to ask whether Mr. Greene's birds were then abundantly supplied with a variety of green food, and with extra supply of salt, which so helps deglutition of the food. I have known the parents kill the birds or neglect them because they could not bring up again the dry dead food for the young, and this more especially with high breeds. My great difficulty, I confess, arose from the jealousy of the cocks towards each other, and sometimes even towards the breeding hens —some of the cocks would fain have kept all the others from tit-bits of food, as though fearful that enough would not remain for themselves and their young ones; and they would devote so much time to watching and hunting the others off that they were apt, through their very jealousy and anxiety, to fail in that which they had in view in fighting to secure all the material for themselves, which they never could have used; and often I had to cage and put the cocks away, and aid, as best I could myself, the efforts of the hens in feeding; and in this I often thought I saw a little parable of human beings and nations, too apt to fight for what they could not use and did not need, and which would either soon spoil or be but a burden to them. Here I am afraid even the breeding canaries suffered from too regular and copious (yet not enough varied) supply of food.

III.

And then only think of a great scientific man who unqualifiedly and dogmatically tells you that "with hardly an exception our domesticated animals, which have long been habituated to *a regular and copious supply of food*" (unqualified, mark you), "*without the labour of searching for it*, are more fertile than the corresponding wild animals," and yet has set down: "It is said that mares brought up on dry food in the stable" (absolutely, you see, without the trouble of searching for it, or even stooping down to pick it up), "and then turned out to grass, do not at first breed."[8]

That should have raised more questions in Mr. Darwin's mind than it seems to have done. Why, there is absolutely "regular and copious supply of food without the trouble of searching for it" in the stable, as there was "regular and copious supply" of grass, we shall suppose, on the meadow where the mare went to grass; but, oddly enough, breeders know that there is a period after such a change when his words and rules as above do not at all apply. But Mr. Darwin glides over it; he does not properly face it, nor explain it, nor even perceive it in relation to the dogma he has laid down as given a moment ago, even to suggest the least modification or qualification. I should be exceedingly obliged to Dr. A.

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[8] "Domestication," ii., p. 151.

Russel Wallace, or the great Mr. Grant Allen, or any other pure or mixed Darwinian, if he will explain that matter to me in a satisfactory way. I am suggesting good subjects for articles and books to them "free gratis, for nothing," but they are welcome, welcome, right welcome!

IV.

Then, again, just take these few little extracts and try to master them as you read:—

"Some characters become attached, from quite unknown causes, more strongly to the male than to the female sex; so that we have, in certain races, a tendency towards the appearance of secondary sexual characters, of which the original rock-pigeon displays not a trace. This term—secondary sexual characters—was used by John Hunter for such differences in structure between the males and females as are not directly connected with the act of reproduction, as the tail of the peacock, the horns of deer, &c."[9]

On the other hand, these secondary sexual characters tend to disappear in certain circumstances. In confinement many creatures lose the distinctive mark of sex, as the male linnet, which loses the bright red. The oriole and the bullfinch have been known in many cases to assume the plumage of the hen. And Mr. Darwin cites some definite cases, and these must be added to the list. It is undoubted that the female wheatear sometimes assumes absolutely the male dress; and as one instance dead against the sexual selection theory out of scores of the same kind—the female blackcap has a rusty-red crown, while the male, true to its name, has a black one. But the female is the sitter here, which adds to the force of the instance.

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[9] "Domestication," p. 179.

> "It is a singular fact that the males in certain sub-breeds [of fowls] have lost some of their secondary masculine characters, and from their close resemblance in plumage to the females are often called 'hennies.' There is much diversity of opinion whether these males are in any degree sterile; that they sometimes are partially sterile seems clear (see Tegetmeier's Poultry Book, 1866, p. 131), but this may have been caused by too close interbreeding. That they are not quite sterile, and that the whole case is widely different from that of old females assuming masculine characters, is evident from several of these hen-like sub-breeds having been long propagated. The males and females of gold and silver-laced Sebright bantams can be barely distinguished from each other excepting the combs, wattles, and spurs, for they are coloured alike, and the males have not hackles, nor the flowing sickle-like tail feathers. A hen-tailed sub-breed of Hamburghs was recently much esteemed. There is also a breed of game-fowls in which the males and females resemble each other so closely that the cocks have often mistaken their hen-feathered opponents in the cock-pit for real hens, and by the mistake have lost their lives."[10]

Is there any law that underlies these cases? Has natural selection, or artificial selection either, justified itself there? Is the loss to the gamecock of male marks anyway serviceable to it, or is this a case of reversion to the time before all such distinction?

When the cocks (hennies) could not tell the difference between themselves and the hens, and "by the mistake lost their lives," it *could not* be said for the life-losers, this was an advantage anyway; but I should like Dr. A. Russel Wallace and the great Mr. Grant Allen to tell me if this was a case of reversion or selection? or, if this is not asking too much, what is the law about losing these secondary sexual characters or gaining them? Such domesticated fowls in both cases "have been long habituated to a regular and copious supply of food without the labour of searching for it," but both classes are *not* "more fertile than the corresponding wild animals." I should be glad to have reasons more relative than Mr. Darwin gives why there should be so great a difference—and on one side wholly against the truth of Mr. Darwin's own dogma advanced above as against the truth of another deliverance of his from another side.

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[10] "Domestication," i., p. 253.

V.

Then again, please read, with reference to birds pairing with birds of another genus, when they do not pair with their own species, or, at all events, are not fruitful with them.

The difficulty with the many finches which have been kept in confinement is all the more remarkable as more than a dozen species could be named which have yielded hybrids with the canary, but with the exception of the siskin (*Fringilla spinus*), none of these have reproduced their own kind. Even the bullfinch (*Loxia pyrrhula*) has bred as frequently with the canary, though belonging to a distinct genus, as with its own species. Bechstein speaks of bullfinches making nests, but rarely producing young.[11]

Now all these creatures, without exception, may be presumed " to have been long habituated to a regular and copious supply of food without the trouble of searching for it," and are not more fertile with their own kind than the corresponding wild animals, though they may be moderately fertile with a different species.

Again, " There are some animals which pair faithfully in the wild state, but become polygamous under domestication." This is true, for one, of the wild drake and the domestic drake. Is this under rule of reversion, natural selection, artificial selection, or what? I shall be very glad to hear (this will be the more interesting to me as I have long been trying to get at the root-rule about polygamy and polyandry in animals and in men and their effects), as also, to the same effect, about the canaries interbreeding with other finches that fail to breed with their own species.

Our principles very directly explain the following cases :—

" All our domesticated productions, with the rarest exceptions, vary more than natural species. The hive-bee, *which feeds itself and follows in most respects its natural habits of life*, is the least variable of all domesticated animals, and probably the goose is the next least variable; but even the goose varies more than almost any wild bird, so that it cannot be affiliated with perfect certainty to any natural species."[12]

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[11] " Domestication," ii., p. 154.

The goose usually goes free—finds a large portion of its own food, which is practically the same as that of its wild congener.

VI.

The difficulties felt with regard to animals are always, by necessity of his theory, pressed up into the human sphere by Mr. Darwin. He persists in applying to the human race the same principles as he thinks he has found operating on the lower creatures all along the line. Here is one utterance of his:—

"It is *commonly believed*" (mark, Mr. D. does not say, it is *scientifically* believed), "that the people of the United States differ in appearance from the parent Anglo-Saxon race; and *selection cannot have come into action within so short a period.* A good observer (E. Desor, in *Anthrop. Rev.*, 1863, p. 180) states that a general absence of fat, a thin and elongated neck, stiff and lank hair, are the chief characteristics. The change in the nature of the hair is supposed to be caused by the dryness of the atmosphere. If immigration into the United States were now stopped, who can say that the character of the whole people would not be greatly modified in the course of two or three thousand years."[13]

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[12] "Domestication," ii., p. 254.

This is so like Mr. Darwin—so very like him. It is now some two hundred and seventy years since the United States were settled by Anglo-Saxons, and allowing fully for immigration, &c., should it not be the most rational of inferences and calculations that if in one thousand five hundred years, say, without immigration, the character of the whole people would be greatly modified, that in one-eighth of that time, when the increase has reached fifty millions, some distinct mark even with immigration amounting to one-eighth of the great modification should be clear and traceable. Or does selection, with Mr. Darwin, begin to act (mechanically, yes, like the starting of a machine) at a point somewhere just beyond the two hundred and fifty years?

Again, if the change to America of animals, birds and plants, from exactly the same places as the men and women had come from, show marked changes and modifications in the course of a few years, as is also the case with plants brought from there (say, maizes, Metzger's maizes, of which Mr. Darwin was forced to take notice), is it reasonable, is it within the bounds of possibility that certain of the physical aspects of men and women should not show some change in general type or character, and not without some play of selection also? Verily, if we were to meet easy writing, hard reading like this in a daily newspaper, we should think it yielded a bad certificate to the common sense of the editor. And is it different because it comes to us over or below Mr. Darwin's name? I refuse to bow to mere authority here as well as elsewhere, and not to be mastered by any such half-thinking, always with a reserve of trying somehow to save an old and most mistaken position, as fully confessed in his "Life of Letters" by this Mr. Darwin himself. Only, I suppose, his publishers having his books in plates could not or would not agree with him to alter or re-write them to the extent that they demanded, if only for honesty's sake and the sale of what was not confessedly adulterated.

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[13] "Domestication," ii., p. 277.

And then Mr. Darwin goes on to try to prove that such modifications as are met with in animals in this case are due not in any form to selection, but to reversion. In all such cases we have to consider, in addition to the tendency to reversion to progenitors more or less remote, the natural selection of the finer shades of difference. If the American tenuity of figure, length of neck, and stiff straight hair is a reversion to progenitors, then these traits in progenitors must have been due to similarity of conditions for one element, and we look in vain along the long line to find it.

In chapter xxiii. of "Domestication," Mr. Darwin puts forth all his power to show that there is a distinct and recognisable line to be drawn between the definite results of the changed conditions and the accumulation through natural selection of serviceable variations which have arisen independently of the nature of the conditions. In most, perhaps in all cases, says he, "The organisation or constitution of the being which is acted on is a much more important element than the nature of the changed conditions in determining the nature of the variation." And I for one say, and say decidedly, that I reject any such distinction forced on Mr. Darwin by his first unfortunate attitude of regarding natural selection as practically independent of conditions, which Mr. Darwin himself plainly confesses in his "Life and Letters" that he had been compelled to renounce, saying that were he then to write the "Origin of Species" *de novo*, it would be a very different book, and yet his blind and half-blind followers and admirers continually want me to close my eyes to that. But if there is the least truth in Haeckel's pronouncement to the effect that certain influences act directly on the organs of propagation, and which leads him to go on: "Every change in the sexual organs reacts on the rest of the body; so, on the other hand, every important change on another part of the body must necessarily more or less react on the sexual organs. This reaction will, however, generally only become perceptible in the formation of the offspring, which arise out of the changed generative parts"; on this a word yet remains to be said.

For then where is Mr. Darwin? The organisation or constitution of the plant or creature is at once affected by the conditions: it is modified, changed, and has accepted new predispositions, new prepotencies, &c., or at least has lost some of the old. If every part—muscle, bone, hair, skin, blood—is dependent on the slightest change effected on any one part, and cannot escape that influence, if the slightest change on any part of an animal directly and more especially affects the reproductive organs, then, I ask, what in the name of common sense, not to speak of *thought*, is the good of telling one that changes of condition though affecting modifications, noticeable and plain, do not affect organisation and constitution? It is illogical, unnatural and contrary to fact. Natural selection, according to Mr. Darwin, is not to act or put in an appearance till a certain definite point of progress. I say that if natural selection is even what he says, it acts continuously and has no definite mechanical lapses or pauses. The mechanical business, the result of Mr. Darwin's unreal and forced distinctions, I, for one, will have nought to do with; and I see perfectly well why he, even down to the date of "Domestication," was so anxious to disconnect external conditions from organisation or constitution—both sides of one and the same problem, acting and interacting constantly on each other, and I do not believe in his "thinking" in that direction as being either clear or good, or apart from a destructive vein of prepossession and of theory—mere empty or mischievous theory. Nature is not a machine that Mr. Darwin, and Mr. Romanes after him, should try to be her master, and lay down mechanical rules for her.

And what am I to think, in face of the laborious effort of chapter xxiii. and its drift, and of many sentences, in "Origin of Species," &c., which surely Mr. Darwin had clean forgotten—even though they still stand and are sold largely by Mr. John Murray, and are to be bought over any bookseller's counter. And though, as I have said, the first hint of Mr. Darwin's great *volte face* is to be traced in "Domestication," he still, without reserve, in his "Introduction," refers readers to the "Origin of Species," and makes there what professes to be a short summary of that work. This was in 1868, nine years after first publication of the "Origin," which proves how slow he was to make full confession, in spite of what is said here and there in the body of the later work.

On the other hand, it is clear from well ascertained facts that cross-breeding in nature is, over large areas, common. Siebold's view is certainly well founded, that the vast number of intermediate forms which constitute the crux of the geologist who endeavours to determine the species of the fresh-water fishes of Germany must have originated from cross-breeding in a state of nature; this naturalist, in his well-known work on the fresh-water fishes of Germany, enumerates no fewer than eight hybrids, most of which have been described by other zoologists even as types of special genera.[14] Something near to uniformity of conditions could not, according to Darwin's dogma, have existed in this area.

If man promotes varieties by artificial selection, he also limits and encloses—puts a bar on the possible tendency to variety in nature; so that the following words are evidently from this side quite true:—

"A breed of exceptional animals, rigorously selected and carefully isolated from admixture with others of the same race, would become shattered by even a brief period of opportunity to *marry freely*.[15]

Shirley is decided that lengthened breeding in and in is disadvantageous; he says, "There can, I think, be no doubt that a judicious cross with a good stock is of the greatest consequence, and is, indeed, essential sooner or later, to the prosperity of every well ordered park." Nature, apparently, has given the lesson which is preached here, in the teeth of Mr. Darwin's uniformity of conditions, and herself gets crosses of a most marked kind.

Mr. Herbert J. Webber, of the United States Agricultural Department, has published some results of experiments in the hybridisation of oranges—the Japanese orange, *citrus trifoliata*, with the common sweet orange—the hybrid being more vigorous than the sweet orange, and the tangerine with the common variety. He found that, in experimenting with pineapples, by the crossing of seedless fruits, they came to produce seeds, and that the plants so dealt with were more vigorous and better able to resist disease.

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[14] Semper, "Animal Life," p. 355.
[15] Galton, "Nature," xxxiii., p. 297.

Dr. Maxwell Masters told at the Royal Horticultural Society Meeting at Chiswick, in July, 1899, that "Dean Herbert, an industrious member of the Society, saw in the Pyrenees a narcissus growing between two narcissi of similar character, and he found that it was a hybrid. Why, then, should he be blamed for doing what nature had done and shown the way in doing? Some have said that crossing should not go beyond varieties, and that species should be regarded as separated from each other by a wide gulf. But what he should like to know was, what was a species? No one could tell him what a species was."

We have made these citations mainly with the idea of here asking the question in how far the new trees, fruits and flowers introduced by man as he advances into new regions may slowly but still surely by means of nature's crossings, modify the life around, which man may not in any conscious way touch or even recognise. One of Mr. Webber's cases was the crossing of a merely wild hedgerow variety or species with a cultivated one, with the result that certain advantageous traits of the cultivated sort were combined with the strength and vigour of the wild sort. And if Dean Herbert's Pyrenees narcissus was what he thought it, and men in crossing are following nature far more closely than generally they know, have we not ground for the question we have asked pointing to man himself as unconsciously co-operating with nature very probably in producing new varieties or species?

Several considerations render it probable that variability of every kind is directly or indirectly caused by changed conditions of life. Or, to put the case under another point of view, if it were possible to expose all the individuals of a species during many generations to absolutely uniform conditions of life, there would be no variability. And yet not very far off most natural species have been habituated to (nearly) uniform conditions of life.[16]

This looks exceedingly like my own position that in conditions lies the primary excitation to modification of structure, of organism, of constitution. And Mr. Darwin, too, when he lets himself go, forgetful for a moment of his own initial blunders and the necessity for watching over himself not to "commit hisself" too plainly, like a certain character of Charles Dickens', has a glimmering of my own idea about the effect of want of exercise on sexual organs and breeding, which certainly is not on the same line at all as the central argument of chap. xxiii. of "Domestication of Plants and Animals."

"In the case of animals the want of a proper amount of exercise, as Bechstein has remarked, has perhaps played independently of the direct effects of the disuse of any particular organ an important part in causing variability. We can see in a vague manner that when the organised and nutrient fluids of the body are not used during growth, or by the wear and tear of the tissues, they will be in excess; and as *growth, nutrition, and reproduction are intimately allied processes,* this superfluity might disturb the due and proper action of the reproductive organs, and consequently affect the character of the future offspring."[17]

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[16] "Domestication," ii., p. 253.

VII.

Much might be said in this place of Mr. H. Spencer's idea of "the antagonism between growth and reproduction" in reference to the statement here that they are intimately allied. That must wait; but meanwhile we may once more quote the following from chapter xxvii.:—

> "Variability often depends, as I have attempted to show, on the reproductive organs being injuriously affected by changed conditions, and in this case the gemmules derived from the various parts of the body are probably aggregated in an irregular manner, some superfluous and others deficient. Whether a superabundance of gemmules, together with fusion during development, would lead to the increased size of any part cannot be told; but we can see that their partial deficiency without necessarily leading to the entire abortion of the part might cause considerable modifications, for in the same manner as a plant if its own pollen be excluded, is easily hybridised, so in the case of a cell if the properly succeeding gemmules were absent it would probably combine easily with other and allied gemmules.

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[17] "Domestication," ii., p. 258.

We see this in the case of imperfect nails growing on the stumps of amputated fingers, for the gemmules of the nails have manifestly been developed at the nearest point."[18]

See here how Mr. Darwin is exactly making towards what we have said of the interdependence of part of an organism with all other parts which in their combination tend to mend up in their own way what may have been cut off or become deficient.

> "Most natural species have been habituated to nearly uniform conditions of life for an incomparably longer period of time than have domestic races; and we positively know that changed conditions exert an especial and powerful influence on the reproductive system. Hence this difference in habituation may well account for the different action of the reproductve organs when domestic races and species are crossed. It is a nearly analogous fact, that most domestic races may be suddenly transported from one climate to another, or be placed under widely different conditions and yet retain their fertility unimpaired; whilst a multitude of species subjected to lesser changes are rendered incapable of breeding."[19]

And so are wiped out without any respect to the fittest among them to survive.

In the first place, looking back at the last extract, we must ask: What has Mr. Darwin here to say of the whole families of migratory birds? Are their conditions of life uniform when they pass over almost half the surface of the globe from a cold climate to a warmer, or from a warmer climate to a colder?—some of them for longer or shorter periods resting by the way. And then in many species more, there are the lesser and shorter migrations, due to causes so subtle that we cannot always be sure of what they are; but of this we may be absolutely sure, that something in the conditions impels them to seek other conditions. All birds move thus more or less; and seek to escape one set of conditions to find another. Many mammals do the same at certain times—wild oxen, wild sheep, wild asses, zebras, horses, and countless smaller mammals — not forgetting land crabs and fishes, notably the salmon from salt to fresh water. "Uniform conditions of life"—that is a phrase well suited there to Mr. Darwin's purpose, but in the absolute sense (in which an ordinary reader would infer it from his words) it literally does not exist anywhere, or at any time, or has ever existed anywhere at any time, and that on his own theory. Geology and physical geography clearly tell that nothing is in the least stable or uniform; but all is unstable, fluctuating, moving; the rains denude the mountains, and, helped by the wind, carry and spread the benignant salt over mountain and plain, but never in the same quantity exactly, and sometimes strangely leaving out a district so largely that all the animals there either sicken or must move, simply to find more salt, and that salt in peculiar action with soil—chalk, iron, lime, &c.—gives the most extraordinary results on vegetation, and what is more, colour and character to the creatures moving on it. We have already referred at p. 7 to the fact that Jersey cattle kept in Devonshire become some shades darker red, like the native breed, while, though Somersetshire cattle are red, they become several shades lighter than the Devon, exactly as they are distant from the red sandstone; the streams carry away the land, the sea eats and is for ever eating away the rocks, and the sand she throws upon the beach is never twice the same after tides. As Tennyson sings :—

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[18] "Domestication," ii., p. 394.

[19] *Ibid.*, ii., p. 411.

> "The hills are shadows, and they flow
> From form to form, and nothing stands :
> They melt like mist, the solid lands
> Like clouds they shape themselves and go."

Now, it is indeed a fine thing to find Mr. Darwin of all men positing anything like or near to "uniformity of conditions" when really he is arguing for uniformitarianism of processes of change, slow and sure, slow and sure, exactly as Sir Charles Lyell went for in his particular line. I have tried to understand many things, but Mr. Darwin I cannot understand. You cannot have "uniformity of condition" and uniformitarianism of process and result too; for the one is based on fixity and the other on fluctuation —it may be slow, but ceaseless—though it may be very, very slow, yet also of a necessity very, very sure. Hegel's Absolute was always a becoming —phenomena, conditions—are always a-coming and a-going; it is because of this that they are phenomena and conditions, and to have to write thus in the year of grace 1899 in reference to the work not only of a great naturalist but of a thinker, makes me rather ashamed of falling back so much and so fully on what I was well taught in the logic and metaphysic classroom of Edinburgh University by the worthy successor of Sir William Hamilton, "forty years ago, my boys, forty years ago."

"Coming events cast their shadows before." The world is now much nearer being occupied than it was; exploration has almost had its perfect work, and wherever man—white man especially—penetrates, it is a necessity of his being if he is to subsist and to multiply, that for hundreds or even thousands of miles he is sending changed conditions ahead of and all around him, just as anywhere every man who digs up what before was a waste, however small, to make it fruitful or bread-yielding, changes the conditions for crowds of creatures of whom he never thinks, and these creatures, many of them banished to other spots, change all the conditions for those that were already there—not a stream that in the slightest changes its course, but changes conditions over a large area; not a change of wind, but changes conditions for crowds of creatures. Change of conditions—that is constant, that is incessant; there is no such thing in heaven or on earth as Mr. Darwin for his purposes tries to posit there. A positive mind, a hard, matter-of-fact mind this? Do not believe it. He is as imaginative and creative as Tennyson himself was, if not more so, in that he does not see it but thinks otherwise, and deceives both himself and others. The negroes' demand for skins to make carosses, as we have seen, ministers to a marked modification of brooding habits in the hornbills or toucans.

Mr. Darwin's mere presence, with his habits on the heretofore unvisited Galapagos Islands, was enough to change the conditions for nearly all the lower creatures there, for, after having found the birds and smaller mammals so tame that with the muzzle of his gun he pushed or could have pushed a bird from a tree bough and held out in his hand a pitcher to which the birds, as he tells, fearlessly came to drink, and afterwards for specimens began to shoot them; and thus left behind him a legacy of fear and terror of man instead of trust in him, as I rather suspect did largely also Dr. A. Russel Wallace in the Malay Archipelago and elsewhere, leaving their mark on those places long and deep, for he told me he was always so busy collecting that some things at least he could not observe. I say nothing of this kind of thing further than this: that a change of the feeling or instinct of whole races of creatures from complete trust in man to utter fear and terror of him is and must be scientifically a considerable change of condition inward—violently reversing instinct, don't you see?—if not outward gradually too, determining movements and other tendencies at least as marked and subtle, but I leave it to Dr. A. Russel Wallace and the great Mr. Grant Allen to tell me whether it is a case of reversion or of selection.

The question of breeding and the possibility of the union of different species producing anything else than sterile hybrids practically goes very deep, and it bears very closely on the question of physiological characters in opposition to mere structural changes or adaptations. Prof. Huxley, in his "Man's Place in Nature," 1864 (pp. 106-7), makes his recognition of this point clear. He says:—

> "If the animated world presented us with none but structural differences, I should have no hesitation in saying that Mr. Darwin had demonstrated the existence of a true physical cause amply competent to account for the origin of living species, and of man among the rest. But in addition to their structural distinctions, the species of animals and plants, or, at least, a great number of them, exhibit physiological characters—what are known as distinct species, structurally, being, for the most part, either incompetent to breed with each other, or, if they breed, the resulting mule or hybrid is unable to perpetuate its race with another hybrid of the same kind. . . . Our acceptance of the Darwinian hypothesis must be provisional so long as one link in the chain of evidence is wanting, and, so long as all the animals and plants certainly produced by selective breeding from a common stock are fertile with one another, that link will be wanting. For, so long, selective breeding will not be proved to be competent to do all that is required of it to produce natural species. I adopt Mr. Darwin's hypothesis, therefore, only subject to production of proof that physiological species can be produced by selective breeding."

A wise and far-sighted reserve, amply justified then, and more so now, by many facts; and yet Mr. Darwin's blind upholders would have us to close our eyes to the points on which Prof. Huxley's were then so open.

My interest in all forms of polygamy or polyandry, as bearing on reproduction, has led me, as I went along, to make note of any peculiar cases. Among these is the following passage from Lloyd's "Field Sports in the North," dealing with the hares of Scandinavia:—

"Although several male hares—ramblers, or dancing-masters, as they are jocosely called in Sweden—often follow the female at this season, her favours are *believed*" (italics are mine) "to be bestowed on one alone. The males, especially the old ones, fight desperately on these occasions, and the strongest always carry the day."[20]

Lloyd also gives cases that seem to point to the possibility in the hare of conceiving a second time before birth from the first conception.[21]

I have now and then heard strange and suggestive remarks about certain habits of the hare with us in mating, &c., but I should be glad if those who have had opportunities of close observation would tell me whether any of their observations would justify one in inferring any similar habit or tendency in the hare in our country to what is clearly polygamy or a tendency either to polygamy or polyandry in the hare of Sweden and the north, according to Lloyd. Is there any ground for thinking that there may be disproportion in the sexes under any circumstances?

And, since I have referred so fully to Mr. Darwin as carrying the principles deduced from study of the lower creatures up into the plane of the human, I should much like to have facts and figures on these points:—(1) are eunuchs longer lived than other men? (2) is it found that in polygamy in human societies stronger children are born of later wives, or not?

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[20] P. 364.

[21] P. 365.

(3) in polyandrous societies (whether communal or fraternal merely), is there any tendency to reduce the true procreative power in any direction, and is there any tendency to the loss of secondary sexual characters, as a markedly less vigorous growth of beard in men, and so on?

POSTSCRIPT.—Among polygamous peoples, such as we have had to do with in the East, a mere general glance seems to suggest an uncommon energy and determination often on the part of sons not by first wives—a matter which is constantly cropping up on questions of succession. How is this, what are the facts, and has any one traced out and scientifically compared and drawn inferences from them? A little note at one place by Sir Richard Burton seems to point in the direction I indicate.

PART V.

LOOSENESSES AND CONTRADICTIONS ON THE PART OF MR. DARWIN AND DARWINIANS IN USE OF THE WORD "INSTINCT."

A DOUBTFUL TERM—INSTINCT—
Which Causes and has Caused great Confusion in Natural History.

I.

It is surely much to be wished that we could get rid wholly of that very loosely-used and much misleading word *instinct*. Not only in the hands of young writers and of merely popular writers, but even with authors professedly severely scientific like Mr. Darwin, Mr. Romanes *et hoc genus omne*, it is loosely used, without definition or adequate distinction, though it is made to cover phenomena that seem when really looked at carefully and disinterestedly studied to have wholly different origins, and, indeed, to be quite different as regards essential aim and end, as well as character. Not a few will regard our position as very risky and bold when we propose to show that Mr. Darwin himself is here a very great offender; but, after much thought and reflection, I have come honestly to think so, and here beg to record some at least of my reasons.

Mr. Darwin's vocabulary here is wide and easy.

He speaks about "incipient instincts," "complex instincts," and these of necessity imply simple and less complex instincts, of "*lost* instincts" and "recovered instincts," and of instincts that have been developed from "occasional habits," and instincts again developed "through the selection of self-originating tricks," of "intermediate instincts," that is, instincts which have been called forth between the loss of a former instinct and its recovery again a considerable time afterwards. He has "original instincts" and "mistaken instincts," "ordinary instincts" and "extraordinary instincts," instincts that are the development of clearly intelligent actions, and instincts that are the outcome of clearly chance actions or accidental discoveries, and instincts that are the results of "mental peculiarities," whatever that may mean in the lower animals. He has "failures of instinct," as in the case of the lemmings for one species, and an imperfect instinct in the case of another species. He has "strange instincts" and "cruel instincts," as in the case of the cuckoo, and instincts that mislead—"misleading instincts." He claps an adjective before the word with the utmost facility, as though that helped to solve the problem.

"Migratory instinct," says Mr. Darwin, "not rarely fails, and the animals are lost. What ought we to think of the strong impulse which leads lemmings, squirrels, ermines, and many other animals which are not regularly migratory occasionally to congregate and pursue a headlong course across great rivers, lakes, and even into the sea, where vast numbers perish; and ultimately it would appear that all perish? The country being overstocked seems to cause the original impulse, but it is doubtful whether in all cases scarcity actually prevails. The whole case is quite inexplicable."[1] And certainly the reason here given is to me inexplicable: the country overstocked and yet no scarcity of food actually prevailing. The one economically annihilates the other; and, mark, it is yet an *original* impulse stirred only by this double and self-annihilating cause.

Now, just look for a moment at the case of the lemmings. Their instinct leads them at a certain time to go *en masse* in one direction straight on till they reach the sea, when they rush into the water and are drowned. I cannot, therefore, see now in that instinct anything that works for the benefit of the species—a point on which Mr. Darwin is so strong —unless over the left, as the wags say; but clearly the lemmings would ere this have been extinct by their own act, unless there was in their action something that told that in former times this instinct was beneficial to them, and that they made that journey, perhaps crossing rivers and lakes, but not needing to attempt the sea. The physical geography of the earth has changed, as it is always slowly, imperceptibly changing, and sea rolls where earth once stood with only a river like a ribbon meandering through, where the ancient forbears of the lemming found home and food. Nor is this so absolutely theoretic, and with no analogy in the actual facts of science. It is demonstrated, indeed, that " Northern Patagonia and a portion of the Argentine region have been raised from the sea during the most recent geological period,"[2] and it is absolutely true that wherever you have upraisal like this, you have correspondent sea depression or gathering of waters where no waters were before. Now, there the inherited instinct is right—absolutely right—not a failure at all ; indeed, it is too right and strong ; only, see you, physical geography has changed, and what the lemmings, poor things, fail in is not in instinct, but in reason, which would suggest their making them adapt themselves better to the changed circumstances, developing an intermediate instinct, and not rush into the sea because their ancient forbears moved and fed exactly there once on the dry land. This Mr. Darwin, if he was at all to characterise it, should have called " blind instinct," uncorrected of reason in any way or shade.

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[1] Appendix to Romanes' "Ment. Zool.," p. 380.

We all know about the land crabs of the West Indies and other places. They make their long journey to the sea to breed—they encounter no end of obstacles, and thousands of them die and form food for the mass that come up behind. Their migration has a clear end and motive, however—it benefits the species by a new generation, and the instinct there is not mistaken. It tells that their forefathers once were sea-dwelling creatures, and they must in their way act on a powerful dominating influence or instinct, and attest it. We can see there clearly the reason and the necessity of it, in view of the permanence of the species. If the poor lemmings had had the least power of reasoning they would have met in council, and looking gravely at each other, have said, "clearly, this long-inherited instinct of ours to move at set times in a certain direction is wrong, because things are much changed there since the habit from which our instinct springs was formed, so much changed that instead of the instinct-prompted action doing good to us and ensuring the maintenance and increase of the species, we shall all be doomed if we go on so, therefore let us pull up, try something else, correct this strong but vague impulse, and act according to reason, that we may live and thrive, and not commit semi-suicide."

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[2] J. Ball's " Notes of a Naturalist in S. America," p. 164.

If one of the great ends of instinct—if not *the* great end—is the preservation and increase of the species, as Mr. Darwin, in more than one place, clearly puts it, then, of course, this is a failure of instinct, or rather we want to press a distinction, which Mr. Darwin misses—absolutely misses; it is a failure of instinct in view of *this* end; but it is not a failure of instinct in view of the aim and motive of the creatures concerned. Mr. Romanes indirectly declared this when he maladroitly, and without clear enough knowledge of the facts, went wondering why nature had not armed the small birds for the preservation of their species against the cuckoo, and in honour of nature's exactitude scientifically measured, declared it was not worth her while, cuckoo impostures being so rare. We do not say that this instinct is mistaken or imperfect, though thousands on thousands die to feed those that come behind them. But this instinct might be made, in Mr. Darwin's words, a mistaken instinct, by certain changes, by retreat of sea and elevation of land just there. The instinct of the lemmings is precisely of the same order—only changes on the earth's surface defeat them, as they may one day defeat the land-crabs; and if the land-crabs do not adapt themselves, but go on in obedience to a long-inherited instinct, driving them in one direction towards what once was sea-coast, they will, of course, meet much the same fate as the lemmings—only it will be as with them, from failure of reason on great physical changes, and not properly failure of instinct at all. No; the instinct was so darn right and unfailing, as a Yankee would say, that it threatened the very existence of the species.

Now, all the gentlemen who follow Mr. Darwin are very keen to fortify themselves by finding what they call "intermediate" varieties, so that they can present, as it would seem, a kind of slow, very slow, preparation for the results or changes that must be admitted. But surely it is hardly possible that here, any more than in the case of the sand-wasp that lighted by mental peculiarity or chance on such a wonderful discovery of paralysing certain larvæ by stinging in the nerves instead of killing them, which this notable one *first* wasp did, as Mr. Darwin says by chance (whether "by mental peculiarity" or mere accident, mark), it can be said that we can get trace of "intermediate varieties" that have, as they say, been "weeded out," so that here, especially in the case of the sand-wasp, you have confessedly the sudden step in one moment made by one individual and somehow, somehow unexplained, utterly unexplained by Mr. Darwin, conveyed to the whole species. This may be "natural selection" and "survival of the fittest," but the logic that is used to justify it is not "selected," nor is it of the fittest, though all too largely it survives.

II.

Mr. Darwin, again, thinks he makes a great point by saying that the instincts of birds are not "unerring," because many swallows get lost in the Atlantic. Here the error is precisely the same as in the case of the lemmings. The instinct which determines the movement in a certain direction is absolutely right in view of aim and purpose in the bird; the erection of lighthouses, the putting-down of lightships, and other causes of accident, the weakness of wing or other things that lead to failure, are surely something very different from the instinct that leads the creatures to pursue a certain course; though on that course outward obstacle or weakness of wing or of breath causes the creature to fail. As well might you say that Andrée's attempt to reach the North Pole was never really made, because he did not succeed in reaching it; through a tear in his balloon the gas escaped, and he came to grief. Instinct, in this case, has to do with purpose and intention, not with opposing obstacles either outward or inward, which work so that that aim or purpose is never realised, but completely thwarted. In cases of the most "unerring instinct," such accidents will occur to individuals of the species, the direction and movement in one way are the main points. To say that instinct is erring because of such risks to individuals is to say that all instinct is something different from what Mr. Darwin assumes it to be; and even in individual cases of succumbing under such obstacles, if we but knew in how far reason may have essayed to correct them we could judge much better.

Again, when Mr. Darwin speaks of the impulse which led the wounded Brent goose to crawl a few miles toward its ancestral feeding grounds, and argues that the same instinct would still lead on the birds over trackless waters, as they retain a true course across the now submerged pathway of their ancient, though not forgotten, land journey, he, in effect, gives himself away in his reasonings about the lemmings. The Brent geese succeed there because they are winged and can fly over the submerged pathway by which their forbears went; the lemmings, were they to try the same course, would be doomed, as they now are, because they are quadrupeds; but in both cases the instinct or impulse to migration is the same, and it is quite right and well directed—only outward circumstances have changed, and by a sort of accident the Brent geese succeed where the poor lemmings, through lack or failure of reason in overcoming and modifying instinct and adapting themselves to circumstances, are lost.

Mr. Darwin holds that the instinct of fear of man in birds and animals is *acquired*, and not original; that birds and innocent creatures which have never seen man are no more afraid of him than are the birds of the cows or sheep in the fields. This instinct of fear of man in these creatures, he argues, is the result, and the result solely, of experience. But, then, the original instinct of trust in man was not acquired by experience. Should not Mr. Darwin, therefore, have made a broad, clear, logical distinction between "instincts," as he pleases to call them, *not* bred of experience, and what he calls "instincts acquired wholly by experience." The two, in origin so different, should not be philosophically slumped under the same designation, however cleverly covered, or attempted to be covered, by a mere adjective stuck in front of it. We want grounds more logically relative than that.

Something might here be said also about the operation of reason in building up what Mr. Darwin was pleased to call an "instinct acquired through experience"—experience of individuals, mark, because it is impossible all members of the species could have at once been subject to the same experience—an experience which, by the way, wholly supersedes, in whole species, an instinct not born of experience. And here, if in Mr. Darwin we had had a thinker, in addition to a most conscientious, accomplished observer and classifier of facts observed, we should have had a very distinct line drawn between the instinct, say, which leads a young nightjar, almost fresh from the egg, on being approached, to close its eyes, as they would inevitably lead to its being detected, and peep through the merest slits, and that which leads an adult nightjar to lie flat on a fence or branch, its feathers and their markings making it almost indistinguishable from the weathered wood or the bark of the branch. In the first, experience can have had no share, and in the second experience may have had a great share. The first is a pure, primary, original instinct, and the second may be very different, nay, must be; for experience, in one form or other, must have come more or less in there. If anywhere you would expect to find what Mr. Darwin calls "incipient instinct" it would be here; but the nightjar just out of the shell acts with as much decision and finish as any creature could show. Instincts not bred of experience and instincts so bred are therefore different wholly.

III.

Mr. Darwin remarks that it is "not singular that the mound-building birds should have lost the instinct of incubation where the sun can be trusted to do all the work instead." This would be efficient if it applied to anything but a wholly limited class of birds ; and the question is why have certain birds resorted to this practice, where others not only still pursue incubation in nests, but construct the most elaborate and artificial of all nests in circumstances, where, on his reasoning, it is absolutely unnecessary; and the labour of constructing nests at all is a thing directly in the teeth of experience, which tells that sun heat, &c., will hatch as well as the elaborate nest-building, the labour of brooding and rearing young being needless either for true individual welfare or for the maintenance or increase of the species? Gradation in position is a point on which Mr. Darwin has laid vast stress. We mean that he is fain to find different species and families of creatures at what may be called different points in the process of reaching one common result. But we do not really see that, philosophically, this very much helps him, since here you have what we may call the very extremes of the scale, without any observable instances of connecting links between them.

Now it is hardly possible that here it can be said that "intermediate" varieties have been weeded out, as both Mr. Darwin and Dr. A. R. Wallace have suggested.

But Mr. Darwin goes ahead delightfully, and so works the word "instinct" that it covers everything. Does a species go and drown itself by large instalments, then that is a *failure* of "instinct," "a mistaken instinct," and so on. Does a species victimise other species and depute to them a large share in the preservation of their own species by killing off a large proportion of the victim's progeny, then that is a "strange" and a "cruel instinct." Does a species of birds abandon nest-building and incubation and build a mound and get their eggs hatched by natural heat, then Mr. Darwin says it is not to be wondered at; putting aside the fact that the vast number of birds under the very same conditions as to heat go on, not only incubating, but building the most elaborate and artificial nests to be found anywhere. If these birds that go on building their highly artificial nests, had, on the other hand, dropped, and were more and more dropping to the level of the American cuckoos, in this respect we could have understood this as a suggestion on a broad scale of his theory of stages, &c., but, as facts are, it is dead against it; and if the birds that hatch eggs by sun heat act on *original* instinct, then what do those birds do that in the same outward circumstances, still continue to build the most finished and artistic nests, when in respect to this most essential process, the sun heat is being wasted, and they put off time and strength in building beautiful and artistic nests, when they might work, it may be, more and larger broods by sun heat or, as an intermediate stage between brooding and sun heat, by fermentation, as some of the mound-building birds elsewhere skilfully do?

IV.

Mr. Darwin's explanation of the power of certain sand wasps in so stinging their prey that they only paralyse and do not kill it, given in effect is this: "The wasp which so stung its prey *chanced, owing to some mental peculiarity or by mere accident*, to sting one on the nerve which governs muscular action, and thus left more vigorous offspring, and this power became inherited because it was found to be of advantage to the species." As against this Professor Eimer argues:—

> "The larvæ of the wasp require animal food. Dead food enclosed in the cell would soon putrefy, living active animals would disturb the egg, and, therefore, the wasp stings but only paralyses grubs and packs them up in sacks like mealies or potatoes, the one after the other in the cell. Now, how did the wasp reach this result—form this habit? At first she most probably killed the larvæ by stinging them anywhere, and then placed them in the cell. The evil effects of this would, however, soon show themselves—the larvæ putrefied before they could be used for food of the larval wasps. In the meantime the mother wasp found out that those larvæ she had stung in particular parts of the body were motionless, but still alive, and then she concluded that larvæ stung in this particular way could be kept for a longer time unchanged as really living stored-up food. She must then have drawn a conclusion by deductive

reasoning. In this case it is absolutely impossible that the wasp could have arrived at this result otherwise than by reflection on the facts of experience."

It is clear that for ages prior to this discovery the larval wasps *must* have been to a large extent fed upon decayed or decaying meat, and must have formed a taste for it with digestive organs become suited to such on the principle of survival of the fittest. If they did not become so suited they perished, and those whose organs became so suited survived. Well, then, when the pioneer wasp or wasps discovered the principle of stinging to paralyse and not to kill, packed up her living stores and began to feed the larval grubs with new-killed food, she would run the risk of injuring the digestion, and, therefore, for a time at least, the species would not be so largely preserved or increased as they might have been without this discovery on Mr. Darwin's own reasoning. "*Chanced*, owing to some mental peculiarity or by mere accident!"[3] That is so like Mr. Darwin. If it was owing to some mental peculiarity, then, with what propriety, scientific or logical propriety, can the discovery be said to be by *accident?* It is owing to a mental peculiarity that Lord Kelvin has made his wonderful discoveries in electricity, &c., but, surely, neither Mr. Darwin would have said, nor any of Mr. Darwin's followers will say, that it was by chance or by accident. Then, think of the nonsensical pleonasm, "*chanced* by mere accident." That is thinking worthy of England. Could the creature have *chanced* otherwise than by accident? *Chance*, in the sense he here means, *is* accident in the sense he here means. And yet his devout followers tell us that Mr. Darwin admitted nothing like *lawlessness* in his great scheme. Here he certainly does not do so if we distinguish logically where he so egregiously failed to do it. There may have been reserved in his mind an idea here of some great cosmic law under which this phenomenon duly ranged itself and was so accounted for; but certainly he does not here bring even that into prominence, and as certainly he does not admit the operation of *law* in any true and philosophic sense within the range of individual action. He says: "The wasp which so stung its prey *chanced* owing to some mental peculiarity" (chance, too, for it was, see you, a chance endowment quite out of the running of wasps, and on his own theory unaccounted for in view of the species) or by mere accident. Well, if that is thinking, I for one, thank heaven, I cannot see it; but I suppose it only illustrates in its own specific way the axiom laid down by some Darwinites, that the growth of the individual is not to be viewed as if it had been proved synonymous with or analogous to the growth or development of the species, which most certainly it is not here when the wasp of genius by chance or mere accident stung in so special a way as to foresee ahead the result—the fullest result—of its wholly by chance or accidental discovery. For, mark you, the first result, even on the most obvious view, would not be beneficial. It would imply too great and sudden a change in feeding for that, unless it also assumed—a big, big assumption—that the by-accident-discovering wasp could also at that very moment, by chance or accident, also keep and lay hold on the exact instant when by "hanging" after slaying the meat would just be sufficiently "high," and no more, no less, to match that to which the larvæ had before been accustomed. And, besides, it has to be noted that the broad result in the benefit to the *species* of this change could only be seen after a lapse of time. The initiative wasp then had such forecast as to see beyond the discovery by chance or accident to the distant results on the whole species. This would, of course, apply only to the by-chance or by-accident-discovering wasp; though, according to Mr. Darwin's own way of putting it above, the individual experience or individual growth here very, very speedily became one with the growth or development of the species which, according to some of his philosophical disciples, if not even constructively by himself, it ought not to do, or to be synonymous with or analogous to it!

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[3] *See* "Organic Evolution," p. 69.

Again, if a "*mental peculiarity*" in one individual can suffice, in effect momentarily, to change the whole condition and habit of a species, Mr. Darwin here puts himself at issue with his own definitions, for the "mental peculiarity" suffices for effects on the species, for which they were in nowise properly prepared, or else the mental peculiarity was in so far shared by all the species—so far inevitably as to enable them to accept at once and to profit by the discovery, so that strictly, after all, the "mental peculiarity" was shared *in degree* by all individuals of the species and was strictly no "mental peculiarity" at all, and Mr. Darwin has there used words loosely and without real meaning or foresight of their direct logical result.

V.

The very careful and interesting observations and researches of Dr. and Mrs. Peckham, of Madison, Wisconsin, "On the Instincts and Habits of the Solitary Wasps," though demonstrating the systematic care of these wasps to sting larvæ in the nervous centres between the sections, do not, so far as we can understand them, quite conclusively settle the matter as they fancy. They write:—

"The general impression that remains with us as a result of our study of these activities is that their complexity and perfection have been greatly overestimated. We have found them in all stages of development and are convinced that they have passed through many degrees, from the simple to the complex, by the action of natural selection. Indeed, we find in them beautiful examples of the 'survival of the fittest.'"

"E. B. P.," in *Nature*, March 16th, 1899, thus sums up the results of their prolonged and careful observations and study:—

"The frequently quoted conclusions that the object of the sting is to reduce the larvæ to helplessness and yet to keep it in fresh condition, that a dead larva would be unsuitable food and an active one a danger to the offspring of the wasp—all these conclusions are entirely disposed of by a few carefully specially directed observations. These show that the larva rapidly dies in a large proportion of cases and yet affords excellent food, and that it may remain sufficiently uninjured to wriggle continuously without stimulation or to move violently when bitten by larva of the wasp."

And he adds:—

"A puncture *anywhere* would produce effect, although probably more rapidly and completely if made in the neighbourhood of a ganglion."

Now, how do these results leave us compared with where we were before? Certainly not much advanced. For these reasons:—(1) If, as E. B. P. says, "a puncture *anywhere* would have the same result, only it would act more slowly," since time can hardly be regarded as a prime element in the matter—the nice discernment and careful procedure of the wasps in stinging at a certain point is thrown away—the most laborious, difficult, and prolonged process really securing no more than the quicker, easier, and simpler one of stinging *anywhere* would secure; (2) even though a large proportion of the larvæ die, and yet furnish excellent food, that is no proof that something may not be gained for the species by this nicer process of stinging over the other and simpler one—the more that a proportion remain "uninjured to wriggle continuously without stimulation or to move violently when bitten by larva of the wasp"; the mixture, in certain proportions of live and dead food, may be the very end desired, and very likely is—else the whole thing would be useless and altogether without end or motive; (3) Dr. and Mrs. Peckham do not seem to us to have made the allowance they should have made for conditions forced on the wasps under their observations and experiments, as proved by the remark in one case that a wasp, to which a certain larvæ was offered, did not seem eager for it, and did its work in an apparently perfunctory manner. As Sir John Lubbock had to confess, some of his experiments were "hard on the ants," so some of Dr. and Mrs. Peckham's seem to have been hard on the wasps. Until due allowance is made for all such circumstances as these, no true result can be attained. "As you were" is really the verdict of Dr. and Mrs. Peckham's experiments. "We have found *them* in all stages of development." If the *them* here applies to wasps, as we take it to do, then that goes against the idea of a perfected species-instinct, though indeed it does the same if that *them* stands for activities; the more that they say "their complexity and perfection have been greatly over-estimated." "Complexity and perfection over-estimated!" Why, what is the first mark of a species-instinct as set down by the devotees of natural selection? This, surely, that through complexity it is perfect in its realisation of its end, though the individual may be without full consciousness of that end. If the aim of the creature, by taking all the trouble to sting the larvæ between the sections, is to secure fresh food for its larvæ, and if a large proportion nevertheless dies, where is the triumph of instinct, developed through natural selection, as we are told, from the simple to the complex? "Beautiful examples of survival of the fittest," indeed! when, according to the doctrine laid down, the "fittest" take a lot of trouble for nothing, and could procure dead meat for their larvæ without any such trouble at all, as their early progenitors once did, or as Mr. Darwin supposed that they did. If so, here you have a partial failure of reason rather than of instinct—the creature failing to learn from experience that it has so far applied a principle which has no real bearing on, or real reference to, its daily needs, or on the welfare and increase of the species. None whatever. That seems to be the position in which Dr. and Mrs. Peckham's reasonings, interpreted by E. B. P., would place us. The one reason for stinging in the nerve-centres was to paralyse, and not kill, so as to have living food for the larvæ. If the large proportion of the larvæ die before being eaten, then, so far the instinct so to sting is made of no effect, and the effort and labour are wasted—reason having failed to correct conduct as suggested by experience, and the "progress" is in no sense beneficial; unless indeed you can see that a great end is secured in the mixture of live food and dead food on the larvæ. Dr. and Mrs. Peckham and E. B. P. seem to miss the all-important fact, too, that always, even in the case of those that die, a greater approach is made to securing fresh meat than would be the case if the larvæ were all immediately killed by stinging.

The notion of first reducing the "complexity and perfection" of the activities of these wasps, finding them "in all stages of development, from the simple to the complex," and then deciding that this is according to "the action of natural selection," and beautifully exemplifies "the survival of the fittest," is plausible, but is certainly not consistent or logical. For, mark you, if still the simplest form of activity (admitting that it can be so fixed and settled) is to be observed in these creatures, what of species-inheritance on any conceivable scheme of it? What of instinct truly conceived as species-inheritance exemplified in action performed for definite useful ends, useful, that is, to individual and species—if at one moment all stages of development "are even" now to be observed and traced; and what of survival of the fittest, if the wasps in the earliest, simplest, and crudest stage here survive as well as those who have so far reached "complexity and perfection," which, of course, is somewhat inconsistently included in the phrase "all stages of development." Well! well! here is another case of valuable observations recorded; but the reasonings based on them made to favour Darwinism, when they are right in the teeth of it—well! well! well!

It is clear that if Darwinism, in the shape of natural selection and survival of the fittest, is aided by Dr. and Mrs. Peckham, Mr. Darwin himself, poor man, gets a slap in the face! His wasp of genius, which, by a "mental peculiarity," or by chance, made such a step as to provide a saving boon for the species for ever after, has no "existents," never had "existents," and is a myth; that the species has never profited by the wonderful "mental peculiarities," in the broad sense Mr. Darwin meant, since now the wasp's activities, or the wasps, are found in all stages of development at once. It is one thing now to aid Darwinism, and another to aid Darwin. *Q.E.D.*

The following letter appeared some weeks after in *Nature*, which, taken in conjunction with the report of Mr. and Mrs. Peckham's experiments, go to prove that Dr. Darwin, just as with Jenner and the cuckoo, blindly accepted a whole set of facts without any attempt to verify or test them and built on them a clumsy argument.

"Perhaps it may interest your reviewer of Dr. and Mrs. Peckham's work, 'On the Instincts and Habits of Solitary Wasps,' to learn that one of the main results in question has been already arrived at in a paper by the late Professor Schiff, of Geneva, in *Memoires de la Soc. de Physique et d'Histoire Naturelle*, de Geneve, vol. xxviii., 1882-3. I quote the following passage, as in some way complementary to the observations of Dr. Peckham :—

'D'ailleurs, un examen microscopique approfondé du système nerveux des animaux intoxiqués par les guêpes n'a pas revélé la moindre lésion dans les nerfs et les ganglions de ces animaux.'[4]—Freiburg, Badenia, March 18th. DAVID WETTERHAM."

So that, while the facts are modified in essential points, the whole acts of the wasps and their effects on the victimised larvæ are really more mysterious than ever, and not less so; Mr. Darwin's wasp of mental peculiarity, or his wasp that first performed the work *by chance*, being more out of place and absurd than ever!

There is another very peculiar instinct—the instinct of the Huanacos—small unhumped camels—to make for certain places, called "dying places," to die; and a great question is why this instinct obtains only at the southern extremity of Patagonia, and not in northern Patagonia, and on the Chilian and Peruvian Andes. It stands alone and apart, says Mr. Hudson, and no sufficient explanation of it has been given.

Then again, the ichneumons furnish a wide field of illustration here,—these graceful creatures, with elegant transparent wings, which they flutter gently as if blown on by gentle winds. The female, by means of a thin thread-like ovipositor, deposits her eggs in a caterpillar *full of health*, and the larvæ, as soon as they are hatched out, find themselves, as has been said, "in a bath of blood and viscera, which serves them for food." These intruders tear, with their sharp teeth, the vitals of their living prison, and grow in size till they are hatched, by which time nothing is left of the victim but the skin in which the young ichneumon is rocked as in a cradle. Not only so. Van Beneden says:—"The young ichneumon devours its nurse piecemeal, organ after organ; and for fear that death should supervene too quickly the mother takes care to chloroform the victim before hand, to make her last longer. The method which many of them adopt to get rid of their young reminds us forcibly of the turning-box in which they used formerly to place children whom they wished to be brought up by public charity; with this difference, that young ichneumons are not only fed and taken care of by some good neighbour, but that her body itself serves them as food. . . . It is not only caterpillars that are sought by ichneumons for the sake of their young; many kinds of larvæ of *Coleoptera* and *Hemiptera*, of aphides and weevils, are attacked by the mother ichneumons, which plunge their ovipositors between their articulations. These winged corsairs well know the weak points of their cuirass." Thus, as well as the solitary wasps, they paralyse without killing their victims, to secure the full and proper food for the larvæ.

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[4] "Besides, a thorough microscopical examination of the nervous system of animals, intoxicated (or paralysed) by wasps, has not revealed the slightest lesion in the nerves or ganglions of these animals."

The *Tachinariæ* act in one respect different from the ichneumons. The female *Tachinæ*, less cruel, go no further than to lay their eggs on the surface of the skin, leaving the larvæ themselves to bore into the interior. But in both cases the question is suggested, what of the healthy and strong insects which have been doomed? The fittest there have no survived, or is it ichneumons and tachinæ here that are to be regarded as absolutely fittest and therefore have survived?

The same problem is stirred by the account of another *tachinaria*. A certain *Sphynx* affects the *Tithymalis* (*Euphorbia helioscopa*):—

> "While this *Sphynx* is still a caterpillar," says van Beneden, "a dipterous *Tachinaria* takes possession of it to feed her young ones. For this purpose the fly establishes itself on the back of the caterpillar, and mounted thus, *without the caterpillar in the least suspecting the danger that it runs*, the fly inserts her larvæ to the number of ten or twelve. When she has thus deposited these, off she goes to seek another caterpillar, like the cuckoo in search of a fresh nest each time that she lays an egg.
>
> "The young flies left to themselves pierce the skin of their host. After three months the fly is completely developed; it then devours the interior of the larvæ which has nourished it, pierces the skin, and the dead body of its host, which might have been its tomb, becomes on the contrary its cradle. While not far off from the remains of its feast its own skin hardens to a veritable shell, and thus protected and furnished with wings, it is ready to carry on the unvarying phases of its parasitic evolution."

Survival of the fittest, eh? With a hint and more of a question we have asked before. Of what benefit to the victim species was this association which in more than one case was actually that of entertaining the enemy?

It is clearly for the advantage of wine-growing countries, and thus for the wine-drinking world, that *Phylloxeræ* can be overcome by introducing there the *Acarus* to do its work; it is for the advantage of cities as to health and beauty that elms and other trees should grow along the sides of the streets; and if the destructive *Scolyti* appear, then you have only to find a supply of the *Bracon iniator*, who can and will destroy them by inserting its eggs in the bodies of the larvæ and eating them up. But, for all that, the scientific problem remains. What about "survival of the fittest"? Must the fittest, in this case, be limited to the species which thus remorselessly kills off the healthiest and strongest of another?

These are certainly very different from the service of that strange contortionist biped or quadruped worm, the *Histriobdella*, which attaches itself to the lobster and eats the eggs and embryo which die, the decomposition of which might be fatal to the lobster and his progeny, and of which van Beneden says:—"These *Histriobdella* have the same duty to perform as vultures and jackals, which clear the plains of carcases."[5]

Here we see mutual benefit, but certainly we can trace none whatever in the former cases we have cited from this field.

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[5] P. 80-1.

On the coast of England lives a certain soldier-crab (*Pagurus Prideauxii*), which has as its principal messmate a sea anemone called *Adamsia*, which Mons. Greef found in the island of Madeira. This *Pagurus* is especially remarkable for the good understanding that exists between himself and his acolyte—he is a model Amphitryon. Lieut.-Col. Stuart-Wortley has watched it in its private life, and thus relates the result of his observations:—

> " This animal after he has fished, never fails to offer the best morsels to his neighbour, and often during the day ascertains if he is not hungry. But more especially when he is about to change his dwelling, does he redouble his care and his attention. He manœuvres with all the delicacy of which he is capable to make the anemone change its shell; he assists it in detaching itself; and if by chance the new dwelling is not to its taste, it seeks another until the *Adamsia* is perfectly satisfied. This association is not confined to the union of a *Decapod* with a *Nereid* and an *Actinia*; a curious *Cirrhepede* often establishes itself on the body of the *Pagurus*, and on the outside of the shell we generally find a colony of polyps, of a rose or yellow colour which extend like a living carpet round this habitation. Thirty-six years ago we have given the name of *Hydractinia* to these polyps, which were till then unknown to naturalists, and which form habitually a double overcoat for the *Paguri*, if I may employ the expression of my learned colleague, Mons. Ch. Desmoulins."[6]

What definite service is here rendered by the *Adamsia* to the *Pagurus*?

Definitions of instinct are not wanting. If they could help us we should be safe. No more trouble then—there is your definition—the facts must square with it, or all the worse for the facts. Here is one from a writer who excels in this exercise and says :—

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[6] Van Beneden, p. 27, in section " Free Messmates."

"Instinct is the name given to those faculties of mind which are concerned in consciously adaptive action, *prior to individual experience*, without necessary knowledge of the relation between means employed and ends attained; but similarly performed under similar and frequently recurring circumstances by all the individuals of the same species."

The instincts—a large class, built on experience—are here, you notice, ruled out.

But, if this is so, just look at the following, and think what it implies :—

"I have been struck by the slenderness of the trees sometimes chosen by the magpie for its nest; but intelligent as this bird is, I cannot believe that it foresees that boys could not climb such trees, but rather that, having chosen such tree, *it* has *found* from experience that *it* is a safe place."[7] Found, how? And what is the *it?*—the individual animal or the species? If the species, how was the discovery of the individual or individuals communicated to *it?* That is the question of questions for the followers of Darwin and Romanes to answer. If the first happened on it, and by memory and something else (what?) so drew inferences from result as to do it again, what faculty was called in there; and what faculty or faculties were exercised to convert the whole species to a sense of the benefit thus first derived from one bird chancing to *choose* to build its nest in a very slender tree? And then by the time the process of species conversion was complete, which surely Mr. Romanes would not have insisted was immediate, what about the "individual experience" by which, as he says, the first magpie that chose a slender tree to build its nest in, was led to continue doing so? Were the species in this case and every individual in it still proceeding prior to individual experience without necessary knowledge of the relation between means employed and ends attained? In a word, do these magpies now act from instinct or on reason founded on experience, or what?

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[7] "Mental Evolution in Animals," p. 370.

Dr. Russel Wallace presents us with his definition thus:—

"The performance by an animal of complex acts, absolutely without instruction or previously acquired knowledge."[8]

This rules out so much in animal action that the sphere of its operation would be limited indeed; but it is due to Dr. Wallace to say that he reserves to animals far more of reason and reasonable action than Darwinians proper do.

VI.

Here is a passage from Major Bendire, one of the most distinguished and thoughtful of American ornithologists—a passage in his clear and masterly article on the cowbirds in Smithsonian Report for 1893 :—

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[8] "Natural Selection," p. 93.

> "Consider the different behaviour of three species that seldom or never warn their offspring in danger : the young of *Synallaxis spixi*, though in a deep domed nest, will throw itself to the ground, attempting thus to make its escape ; the young of *Mimus patagonicus* sits close and motionless with closed eyes, mimicking death ; the young of our common *Zenaida* (Mourning Doves) even before it is fledged will swell itself up and strike angrily at the intruder with beak and wings, and by making so brave a show of its efficient weapons, it probably often saves itself from destruction. But anything approaching the young *Molothrus* is welcomed with fluttering wings and clamorous cries, as if all creatures were expected to minister to its necessities. . . . The young *Molothrus* never understands the language of its foster-parents as other young birds understand the language of their real parents."

And Major Bendire tells about taking a young *Molothrus* out of a nest, knocking it about in every possible way consistent with not really injuring it permanently, and then immediately on putting it back in the nest offered it its proper food, which at once it took greedily from him, as much as he could give it.

Now, the question here for the followers of Mr. Darwin is to tell us which of these birds, the three first-named or the *Molothrus*, acted from proper original instinct and which from a secondary and derived instinct. One or other of them must have done so, since in identical circumstances they acted in a manner totally diametric. The *Molothrus* in thus showing no fear, though an alien and an intruder, did he act in the manner most conducive to his own safety and success and that of the species or did the others do so? Two different diametrically opposed courses of action surely cannot be said to trace to the same impulse—not quite logically, at all events. Was not *Molothrus*—the alien and intruder—from lack of the instinct, or whatever you may call it in the other three, more than necessarily exposed to danger, to destruction, and therefore to decrease of the species, or were the other three in this position? and why, if nature had armed the *Molothrus* with such marked gifts for imposing on the foster-parents, she should not have armed it with some at least of the activity to escape from enemies of *Synallaxis spixi* or the mimicking power of *Mimus* or the pugilistic propensity of *Zenaida*, which the young of our *Cuculus canorus* seem to share. And why should the young of our *Canorus* resemble *Zenaida* here and the young of *Molothrus* be so absolutely in this unlike them both? Which was the true instinct, I ask, and which was the something else? I appeal to the followers of Mr. Darwin for a definite answer to this specific point—this and meantime no other.

We have the authority of a distinguished and observant ornithologist, Mr. P. H. Emerson, for saying that seldom, if ever, is the wheatear struck down by the hawk; it preserves its presence of mind, and by doubling and dodging, generally escapes. Now, why is this power of presence of mind and ability to dodge and double so markedly given to this species, while it is so entirely withheld from others of the little birds not less swift, who clearly are victims because really of panic or fear or something like fascination? Can this be satisfactorily accounted for in view of terms of natural selection, explaining clearly why this bird is so specially armed by instinct where so many others absolutely fail from want of the same instinct, qualities of presence of mind, &c.? Do we have here a case of survival of the fittest in a whole species over all other or most other small birds so far as the race of hawks is concerned?

VII.

Then there is the case of the New Zealand kea and the extraordinary habits it has developed within the memory of living men—wholly in contradiction of all its earlier experience, so far as we have any means of knowing or tracing it.

Mr. Taylor White, in *Trans. of the New Zealand Institute*, wrote from many years' experience about this remarkable bird. He tells us that it lived mostly on the lichen growing on the rocks, and on grubs when obtainable. The lichen would, in places where the rocks were steep, be free from a covering of snow when all else was snowed up to a depth of three feet or more. They live above the true forest-level, moving among rocks and boulders on the ridges near the mountain-tops.

"On first becoming acquainted with man they showed no fear whatever. In fact, I have, when on the mountain-top, remained standing perfectly still, with keas hopping round me, but would have ultimately to drive them away, for one would come to inspect the brass eyelets in my boots, and try to pick them out, when seeing that the boot laces would quickly be cut through, I would require to stop this performance. One that was captured and kept showed an extraordinary gift for boring holes in the box it slept in—biggish round holes, till finally it loosened the chain from its socket and went off chain and all. They began their sheep-killing by drilling a small hole right over the shoulder blades and sucking the blood. As to its victims they were generally merino (long-haired) sheep—the conclusion being that the bird in its process of hole-drilling found a firmer holding for its claws in the long wool, and that point was chosen simply because it afforded a good holding-on position—blood rather than flesh being what the bird desired ; for no carcase was ever eaten, though the birds might be playing round in scores."

Mr. Potts—a life-time resident in New Zealand—says : "It (the kea) homes among the holes and fissures in almost inaccessible rocks in a region often shrouded in dense mists or driving sleet. It has not been known to descend below an altitude of 4,000 feet. It nests in a kind of community in holes caused by layers of rock not falling evenly on each other, leaving a kind of irregular covered-in chambers, and repeatedly Mr. White found small colonies of them thus dwelling together, and the life in important points communistic."

Mr. Huddlestone says : "I have . . . noticed that after a time the old birds leave the half-grown ones to hatch out the late eggs, all the community doing their share of feeding the young. The same habit I have noticed in the native parakeet. The kea's egg is white, and about the size of a pigeon's, but rounder, and with a rough shell. The young birds do not come out of the nest until fully fledged and able to fly. The young birds are so tame that if a person comes across a flock of them and keeps perfectly still they will walk up to him and pull his clothes."

Here is an account of the kea furnished by a Darwinian writer :—

"It (the kea) belongs to the family of Brush-tongued Parrots, and naturally feeds on the honey of flowers and the insects which frequent them, together with such fruits and berries as are found in the region. Till quite recently this composed its whole diet, but since the country it inhabits has been occupied by Europeans it has developed a taste for a carnivorous diet with alarming results. It began by picking the sheepskins hung out to dry, or the meat in the process of being cured. About 1868 it was first observed to attack living sheep, which had frequently been found with raw or bleeding wounds on their backs. Since then it is stated that the bird actually burrows into the living sheep, eating its way down to the kidneys, which form its special delicacy."

Now, here how nicely an attempt is made to get some kind of preparation in the sucking of honey or juices, whereas Mr. Taylor White distinctly says that it feeds on the lichen growing on the rocks and on grubs, being above the flower level.

Indeed, this whole Darwinistic version of things is denied by these early settlers. They say that the bird does not eat the flesh, or, at all events, does not begin with that; that the statement about its having begun by picking sheepskins hung out to dry, or taking meat in the process of being cured, has no foundation in fact; that the birds have not been seen eating any flesh of any kind whatever. They suck the blood, and, so far as is known, it is quite wrong to say that the bird bores down to the kidneys, which "form its special delicacy," for it does not eat them, though it does bore down to a certain depth; this is simply to enable it to reach more full-yielding blood-vessels. They are, in fact, blood suckers, not flesh eaters.

Yet Broinowski, in his admirable book on the "Birds of Australia," with its wondrously accurate and beautiful large coloured illustrations, distinctly says that the keas eat pieces of dead mutton and pick the fragments off the sheepskins hung out to dry, &c., and directly in face of the experiences of Mr. Taylor White and his friends, as we have already told. He mentions that these keas are semi-nocturnal in habit, which would look odd in honey or nectar sucking birds were it not that they are also insect eaters, and that, of course, some of the plants there, as well as here, are fertilised by night-flyers. Broinowski writes :—

"The kea in summer lives abundantly on the honey bearing flowers of the region so long as they are in season. When these disappear to give place to the berries of certain trees and shrubs, it still finds the necessaries of life attainable. But when the hard Alpine winter sets in, and the mountains are snow capped, supplies either decay or are hidden under snow. Then the kea finds it must descend to the lowlands in search of subsistence."

The whole of the Darwinistic elements being here again introduced, and no mention made of the fact that lichen is the staple food, with such insects, grubs, &c., as may be found on the heights.

Dr. Russel Wallace proposed to Mr. White whether it was not possible that the keas had at first mistaken a dead sheep for what is called "a vegetable sheep"—that is, a peculiar growth of lichen. But Mr. White replied that he had never seen "a vegetable sheep" in the part of New Zealand where he was; and as no "vegetable sheep" were there, the keas could not have mistaken it for a living one any more than he could. "I have never seen a 'vegetable sheep' or mistaken such for a living one, and besides, *I never knew the bird to be seen feeding on a dead sheep.*"

Here then is a problem or series of problems, involving questions about instinct, and about the possibility of animals completely changing their habits within a very short period of time. The problem is made all the more difficult that precisely the same results are found in widely separated parts of New Zealand, where it is hardly possible that the keas could have communicated to each other in any way. In some districts where the kea has developed in exactly the same direction there *are* "vegetable sheep," so called, on which the keas feed; but the possibility that the keas could have made a mistake and taken a dead sheep for a vegetable one is ruled out absolutely by Mr. White's experience, and Mr. Huddletone's.

What then is the explanation of this remarkable phenomenon? Shall we say, as Mr. Darwin did, that a kea with a "mental peculiarity," by chance or accident, dropped on to a sheep's back, and boring or drilling a hole there, as it would have done in a board confining it, discovered that sheep's blood was nice and strengthening, and sure to be in favour of the species, went and told all the rest about it—that, odd and extreme as it looks, would only be analogous with his own explanation about the larvæ-paralysing wasp.

Or is it possible to find out, in the case of the kea, any tendency in ages past to bore into the back of any other creature and drink its blood? This is the solution Dr. Woodward has offered; this is the solution which the Darwinians need to seek and to find. This is what Dr. Russel Wallace was anxious to help by tracing the change to a mistake on the kea's part, confounding vegetable sheep with the true *moutons!* Dr. Woodward suggests that the kea proceeds with the sheep exactly as long ago he had done by the *Dinornis*—that he has bled the *Dinornis* in exactly the same way—and when the birds saw the sheep they proceeded to do exactly by them as aforetime they had done by the *Dinornis.* The difficulty even thus is removed but one step further back—how did the frugivorous brush-tongued parrot get to go for and bleed *Dinornis*—by "mental peculiarity" of one individual, by chance, or what? and would not long-memory be as wonderful there as anything else? But no proof is possible; this is the merest guess, the merest fancy of Dr. Woodward, though a guess and a fancy of the kind favourable to Darwinism, as supplying the kind of steps, or intermediate links, which they so delight to find, and on which they are so very fond to found. But some points in the analogy would not hold. The *Dinornis* was not an artificially-tended, or so far an artificially-fattened, creature; and even if the keas of long ago jumped on its back and sucked its blood, why should they have waited till white men came with their artificially-tended and fattened up sheep and then so decisively have gone for them? Instinct, at some point, must have been very decisively crossed by the process; the kea, before the introduction of sheep into New Zealand, must have fed and flourished on something; why does it neglect, or in great part neglect, that something now by devotion to warm sheep's blood? and how did it, mainly a frugivorous creature, come to make such a decisive, resolute, and unhesitating change to blood-sucking, as so many higher intelligencies have done, both at home and abroad? We do not say that the kea is an aristocrat or banker (money lender) among the fauna or avifauna of the antipodes; but he is, like most of them, a blood-sucker, who puts besides a severe tax on labour and the land; not having the true nobility there, the kea comes in to remind the settlers that nature has her own aristocrats and blood-suckers.

Our theory about the kea would be this:—If it is not a nectar or honey-eater, it is a plant-eater, and such birds also are apt to take, as Mr. Darwin would say, now and then insects, that are also after feeding on the plant. They thus come to form a taste for insects; nectar-eaters are all inclined to puncture and bore holes, the quicker to help themselves to the nectar. Being a fearless familiar bird, and often near to the sheep, the kea would discover that insects are apt to harbour in the wool and in the skin. Having been led to the sheep thus, as our starlings are, it would at last find a case in which the insects had got beneath the skin, as they are so very apt to do with sheep, and following the insects down, it soon got a taste of sheep's blood, which it liked, being warm and sweet, and so it learned to bore into the sheep, and to find the blood, which it finally came to prefer to insects or nectar either. We make a present of this to the Darwinites generally, free, gratis, for nothing, to help them out of their dilemma. 'Tis not so far behind Dr. Woodward, any how.

Professor Semper says distinctly that the kea is a blood-sucker, and that only.

"A very interesting example is afforded by the kia [as he spells it], *Nestor mirabilis* [should it not be *notabilis ?*] of New Zealand; it is allied to the parrots, and formerly fed on the juices of plants and flowers, but lately it has become accustomed to sipping the blood of living or newly-slaughtered sheep, and it is asserted that this bird, originally so harmless, has actually become a serious foe to the flocks of New Zealand by its constantly increasing love of the blood of sheep, for it even pecks and sips the most minute wound on a living sheep, and so sets up an irritation which not unfrequently leads to the death of an animal."[9]

Professor Semper would here give the impression that the kea does sip the blood of dead sheep; Mr. Taylor White and Mr. Huddlestone distinctly say that it never feeds on dead sheep, but only sucks the still warm blood of the living.

VIII.

Being thus led to nectar-eating and insects and the boring and puncturing of flowers, we not unnaturally are drawn to dwell on another remarkable case.

In the *Zoologist* for January, 1896, a most interesting and valuable account was given by John Lowe, M.D., F.L.S., of observations of blackcaps, garden-warblers and tits cutting holes in the calyces of certain exotic flowers and afterwards actually tearing the calyces right across, not with the notion of sucking the nectar themselves, but rather with the idea of attracting ants and small insects on which then they fed, taking regular rounds to snap them up as they gathered. The whole is so exceptional, surprising and interesting, that unless it had been presented to us by a tried observer and naturalist it would have seemed utterly preposterous and incredible. But Dr. Lowe observed his facts carefully over and over again, and not only in one place, but in several—in the Canaries and in Algeria.

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[9] "Animal Life," p. 62.

One of the plants so treated by these birds was *Hibiscus rosa sinensis*, a large bush, which there in March is covered with brilliant scarlet blossoms. "These being situated only a few feet from the balcony" [of the Marianez Hotel, Orotava, Teneriffe] Dr. Lowe says, "one could when sitting there observe with great facility everything that passed on them. They were, at the time of my visit, a favourite resort of the blackcap warblers (*capo nera* of the Italians), which came there to feed, taking but little notice of any one who might be on the balcony."

On examining, Dr. Lowe found that the two upper segments of the calyx had a small piece bitten out, leaving an oval opening, while in other cases the calyx was torn right across, and under each of the two upper segments there was found a drop of slightly sweet watery fluid. There was no semblance of a distinct nectary or of a glandular structure. The fluid was not found under any of the other segments, nor was it present in flowers growing low down on the bushes and it was also absent from the flowers of a pink variety which grew near.

The object of this piercing and tearing was not the nectar, but by the exposure of nectar to attract insects. " Several times I observed the bird seize an insect in the act of visiting the pierced calyx, which after being bitten showed a moist spot. When this dried up, I observed the bird tear the calyx across, so as to cause a fresh exudation of moisture. All the newly expanded flowers had the oval openings in the calyx unlacerated. Two species of insects only visited the calyx—a small species of bee and a wasp—and they flew direct to the calyx without visiting the flower. No flies of any kind came and only a species of ant, which evidently was after the aphides living on the calyx. Only a small species of *Bombus* visited the interior of the flower, and this always went straight to the flower itself and never attempted the calyx."

Dr. Lowe also tells us that he found near Las Palmas the *Abutilon*, another plant of the *Hibiscus* family, was visited in the same way by the *Capirote sylvia atricapillæ*. In another case the openings were made by a titmouse, *Parus teneriffæ*, which cannot be regarded as a nectar-eater. This was done to attract ants. These, after the calyx is torn (never before, owing to the pendant character of the flower) enter in great numbers, and after feeding on the nectar are semi-torpid and thus fall an easy prey to the tits, which visit all the flowers at short intervals during the day and clear off the ants. Another bird resembling the willow wren, which builds its nest in palm and ficus trees, also makes periodical visits to the *Abutilon* flowers to feed on the ants. Canon Tristram has named this *Phylloscopus fortunatus*. Later in the day the titmice were to be seen vigorously tearing the calyx with the object of producing a fresh exudation to attract the ants, the supply of nectar having been exhausted.

The same thing exactly was observed in Algiers, writes Dr. Lowe :—

"Only it being cold wet weather at the first stage of flowering no insects were seen, and no birds mutilated the flowers—the moment fine weather came with insects, they began to do so—the blackcap, garden-warbler and a parus, closely resembling *P. teneriffæ*, were seen. The first was observed tearing the calyx of *Abutilon* which had hitherto shown no trace of laceration : a day or two subsequently nearly the whole of them were torn on all the trees, with one exception. This was a large coarse-leaved variety with yellow flowers, which I found entirely devoid of nectar."

In the case of *Gladiolus* (*antholyza*) *æthiopicus* the process was this :—Beginning at the lowest flower the garden-warbler was seen to pick out a small piece at the base of each corolla tube in turn as it ascended the stem. The hive bees were attracted to these openings and fed there, seldom if at all entering the corolla. Dr. Lowe gives a drawing in illustration of this, which makes the point clear. No native plant is thus dealt with. Now, by what process of instinct or of reasoning, by what gradual steps did these birds reach this great and marked change in habit? Who taught them that insects are not abroad in wet and cold weather, or till there is some glimmer of sun? Was it instinct, primary and strong, or reason based on observations, or what was it? there seems to have been no hesitation in their actions here.

Dr. Lowe asks:—

> "Are these openings made for the purpose of obtaining the nectar? To a certain extent this may be so. It shows a marvellous instinct in the blackcap that it can find out the exact spot in the calyx under which the drop of nectar lies; and if to obtain this is its sole object in making the openings, it is difficult to understand why it should, later in the day, tear the calyx across. This could only cause an exudation of sap, which might attract insects, but could not serve as food for the bird. . . . . I certainly think it very probable that the garden-warbler punctures *G. antholyza* for the purpose of obtaining the nectar in the first instance; but in the case of *Abutilon*, I cannot believe that *Parus* would search for nectar; and very clearly, I think, the laceration of the calyx was made to enable the ants to reach the nectary. *Phylloscopus* may be a nectar-eater, but this is doubtful. It certainly does not tear the flowers, but only acts as a poacher, visiting the flowers torn by other birds for the purpose of taking its insect prey. The punctures are made only by the male blackcaps. All the plants punctured are exotic, and have only been introduced in comparatively recent times, *Hibiscus rosa sinensis* from China, *Abutilon* from South America and *Aloe vulgaris* from the West Indies, so that there is the strongest grounds for believing the habit to be an acquired one."

Puncturing flowers is common with nectar-eating birds, as said already; but nectar-eating none of these birds properly are. How then did they come to learn so many peculiar facts about these exotic flowers? There is not here any more than in some of the other cases, any intermediate species or customs or tendency long-continued that would in the least seem to prepare for this. How then account for it on Darwinian principles—how reconcile this sudden and very remarkable departure with evolution or natural selection? The departure so suddenly made, is as yet wholly unaccounted for, by preparation, by intermediate species weeded out or anything of the kind. What do the Darwinians say, how do they explain on their own theories? We shall be glad to hear.

"With regard to the origin of honey-bearing organs or nectaries," says Prof. Henslow, "there are many reasons for inferring their existence to be due to the direct irritating action of insects themselves when searching for juices as food or otherwise."[10]

But how, oh how, does it come that birds—blackcaps particularly—have learned to bring the insects to suck a flower for their own purposes and not Prof. Henslow's?

IX.

Mr. Romanes speaks of "fortuitous variations for natural selection to seize upon," and not only so, but "instincts that do not require to wait for natural selection themselves supply variations, which, in relation to any change of environment, are from the first adaptive."[11]

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[10] "The Making of Flowers," pp. 107, 108.

Instincts that do not require to wait for natural selection—pray which do and which do not?

But what really does "fortuitous" stand for here; is it something not normal or not accounted for by well-traced causes, or what? I should much like to know. I am not aware of any assumed good ground for such a phrase which was not over and over again repudiated by Mr. Darwin in his later years, though he had himself used the phrases "chance," "happened," "accidental," &c., exactly in the same sense as, I presume, Mr. Romanes uses "fortuitous" here, then in effect repudiated them, and tried to define "chance" (most maladroitly, however), in such a way as to make an appearance of saving his consistency. Did, for instance, this conduct of the blackcaps, garden-warblers, and tits come under the list of "fortuitous" variations of habit, laying the foundations of something yet more definitive, or did it not? We wait for a reply.

Why, even after we have got definitions of instinct from men like Mr. Romanes, which they are fain to make cover all the phenomena, presto! comes a case that seems all outside it again, which they incline to refuse to accept as a case just because it seems to disturb and upset their definitions that they had had so much trouble in making, and just, as they say, a mother is fondest of the child that from its weakness, &c., gives her most trouble; so definition-makers are mostly fondest of those definitions which through native weakness caused them the most trouble.

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[11] *Fortnightly Review*, xxxviii., p. 94.

Mr. Romanes tells us, for instance, that he "observed jelly-fish crowding into the path of a sunbeam shining through a darkened tank, and saw that they did so in order to follow the crustaceans, on which they feed, and which also seek the light."[12]

Now, you could *hardly* have exactly the same conditions in nature as you have here—"a darkened tank with a sunbeam shining through." The instinct, therefore, adapted to action in these special circumstances could not have been developed in nature, the exact conditions you have here being there impossible just as Sir John Lubbock admitted, that for the same reason some of his experiments on the ants were hard on the ants. Here, we are reminded, too, of Dr. Russel Wallace's very decided expression on this point, when he dealt with the assumption that varieties occurring in a state of nature are in all respects, analogous to or even identical with, those of domestic (or artificially kept) animals and are governed by the same laws as regards their permanence and further variation, and he goes on to add that it is altogether false, and his illustrations of the way in which the increase or even the existence of a variety is affected by the risk of failure or of great scarcity in the food supplies is, to us, wholly convincing. Though, be it noted, Mr. Darwin has himself confessed that the "Origin of Species" was written before he had come in any effective way to recognise this point, which we are at one with Dr. Russel Wallace in regarding as of the very utmost importance; and the fact—the broad fact—not being recognised by Mr. Darwin, it is impossible he could have distinguished as he ought in the line Dr. Wallace here admirably indicates.

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[12] "Mental Evol. of Animals," p. 258.

Then the question comes, could you have race on race of jelly-fish in unbroken succession confined exactly as here in "a darkened tank with sunbeams shining through," so that this express and exceptional instinct could have been developed in confinement—in long continued artificial conditions as here—one of the most essential elements for development in this way, being, as Darwin so often insisted, a long, long time? If this is not regarded as feasible, then, what I have to ask the friends of Mr. Romanes is this: Is there any escape for you, when you really think of it, from admitting that this was an act of reason on the part of these jelly-fish, since Mr. Romanes says it could not be mere reflex action, and since we have proved that it could not be long-derived instinct from exactly similar operations in free natural conditions, because of the artificial circumstances and the impossibility of jelly-fish for many generations in succession being kept and propagated in precisely the same circumstances as in this tank?

With reference to the jelly-fish moving from one side to the other the dependent organ in its stomach in which are the nutrient fluids on which its young feed, Mr. Romanes says that "*if this occurred in one of the higher animals* it would properly be called a case of instinct; but as it occurs in *so low an animal* as a jelly-fish, it is quite impossible that intelligence can ever have played any part in originating the action." And all I can say is that Mr. Romanes has no right whatever to lay down this line of "high and low" to bar out as he does by dogma a true study of the creatures.

Mr. Romanes, in the same way, disposes of the larvæ of the caddis-fly with reference to its habit of finding floats and air-bladders when its little tubular subaqueous boat is getting loaded, or a stone cemented in for ballast if it is becoming too light, by saying: "It seems impossible that an animal *so low in the zoological scale* can ever have consciously reasoned—even in the most concrete way—that some particles have a higher specific gravity than others, and that by adding a particle of this or that substance, the specific gravity of the whole structure may be adjusted to that of the water. Yet the actions involved are no less clearly something more than reflex; they are *instinctive*, and can only have been evolved by 'Natural Selection.'"

Well, if the action *in this case*, is instinctive, and evolved by "Natural Selection," why should not the action of the jelly-fish just referred to be classed also as instinctive? Mr. Romanes's dictum is that "as it occurred in so low an animal" it is thus sufficeingly ruled out; but we take leave to say that Mr. Romanes here, exactly as in the case of the cuckoo, to his own satisfaction, gets over the difficulty by purely dogmatic assumptions, and therefore fails to reason; though even these assumptions do not lead us to much advance scientifically, because, as he says, both these creatures are so low in the scale—query, at the same point of lowness or not?—but this excellent consistency of reasoning is seen in this that though both are "*so low in the zoological scale*," the one can act by instinct and the other cannot. We want new light and a new line there, anyhow.

Mr. Romanes, having thus decided against the jelly-fish and the caddis-fly as absolutely "too low in the zoological scale" to be allowed the slightest glimmerings of intelligence or reason, shows some slight inclination to make up for it by favouring the Annelidæ, if not at their expense, then at the expense of something. He writes:—

"The Annelidæ I place upon a still higher level of consciousness, because both from the facts mentioned in 'Animal Intelligence' and from those published by Mr. Darwin in '*Earthworms*,' 1881, it seems certain that their actions so closely border on the intelligent that it is difficult to determine whether or not they should be classed as intelligent."[13]

And all this in face of facts that amply prove traits as remarkable in some of the creatures the very lowest in the zoological scale. Why, did not Engelmann most conclusively demonstrate that mere protoplasmic unicellular organisms distinguished colours and unmistakeably showed particular preferences for certain rays of the spectrum?

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[13] Romanes, "Mental Evolution in Animals," p. 77.

My own ideas on the matter very nearly coincide with what is expressed in the following sentence:—

"Intelligence is, after all, in most cases, the guiding principle of life, supplementing and modifying habits to bring them into closer harmony with the environment, and enlivening every day with countless little acts which result from judgment and experience, and form no part of the inherited complex instincts."[14]

Mr. Coe, in his "Nature *versus* Natural Selection," well says:—

"Before we draw a definite line, and say with confidence that intelligence is only found above a certain grade of organic development, we must be sure that the facts of actual experience justify the assumption. Mr. Romanes says that the caddis-fly is too 'low in the zoological scale' to be intelligent. The caddis-fly belongs to the class of insects. Are we to understand Mr. Romanes to assert that insects are too low in the upward path of development to be intelligent? It belongs to the order Neuroptera. Does Mr. Romanes maintain that this order is unintelligent though other orders are intelligent? If so, dragonflies, may-flies, scorpion-flies, ant-lions, and termites are unintelligent," while it is, mark you, an open question about earthworms.

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[14] W. H. Hudson, "The Naturalist on La Plata," p. 210-11.

We here absolutely agree with Mr. Coe as against Mr. Romanes, whose wondrously discerning and far-sighted "low in the zoological scale" is a simple formula to enable him with a *show* of science, to get rid of the real problem before him. Dr. Carpenter at pp. 42-3 of his great book gives an account of how certain of the amœbæ, mere jelly-specks, from sand and quartz-grains, make the most lovely structures, showing the nicest discernment as to the value of different grains for their purposes, choosing this, rejecting that, which in the eyes of Mr. Romanes's friends through the most powerful microscope would present no difference whatever. To say that in this case there can be no intelligence, consciousness or intention of its own on the creature's part is, in our idea, to deny to nature the power to vest intelligence in any creature with organs different beyond a certain degree from the human, whereas, we know from ants, &c., &c., that this is not the case—intelligences may exist not fitted at all with the same organs of perception we possess, and to deny it, and to draw a line and below that line, in face of facts, to say the organism is too low, is merely with a mighty show of reasoning, to beg the question and not to answer it, precisely, as in the case of Mr. Romanes and the little birds not being counter-instincted against the cuckoo; cuckoos' eggs being of so *rare* occurrence.

X.

Mr. G. J. Burch, M.A., in an article on "The Language of our Domestic Fowl," in the *Zoologist*, 1895, pp. 271-2, had some fresh experiences to give. Having been, through the negligence of the hen, obliged to undertake the care of some young chicks, from the time of their leaving the shell, Mr. Burch found what he hardly expected, from having been reiteratingly told about instinct and inherited habit, &c. He found all his preconceived notions of inherited instinct must give way before the utter helplessness of the chicks. Unable at first to balance themselves on their feet, repeated efforts had to be made before they could stand. They were without any notion of picking up food when hungry or swallowing it; indeed, the most rudimentary actions of life had to be learnt by experience. Absurd attempts to reach objects were made before any idea of distance was gained, and stranger still, the "clucking" of the hen only excited fear.

Here is a change of condition that actually made an end of the strongest supposed instinct—answering of chickens to the cluck of the mother hen.

Now, how do the Darwinites account for or explain these facts? According to them the chick should not be so helpless and obtuse as this. "The clucking of the hen only excited fear." Surely that is a mistaken statement—if it is not, then is it lack of instinct or mistaken instinct, a "strange," a "misleading" instinct, or what is it? Surely, surely Mr. Darwin did not, in his scheme, leave a line open just to embrace this! If not, then where is it—what is it—how does it come in under evolution and natural selection?

Sir John Sebright, in his essay on "Instinct," writes —and on many sides direct to our point:—

> "Domestic animals will be found not only to have lost many of the propensities that seem to be characteristic of their species, but to have acquired others that are never seen in the same species in its natural state. These propensities cannot therefore be instinctive, neither can they be the result of experience or of instruction, because they are found in animals that are too young to have been influenced by either. . . . . Nothing could be more different than the propensities of the wild and tame breeds of ducks, rabbits, pigeons, tame turkeys and the wild turkeys of America, the house cat and the mountain cat of Scotland. If the eggs of the wild duck be hatched by a hen, although the young ones will from the first show signs of wildness that are never seen in the domestic breed, they may, if *pinioned*, be made apparently tame, but if suddenly alarmed, or if removed to a strange place, their natural wildness will show itself.
>
> "I bred a duck from a wild mallard and a female of the domestic breed. It appeared to be perfectly tame as long as it remained at the place where it was bred; but on being taken to the residence of a friend to whom I gave it, it flew away. It frequently alighted upon the ponds in the neighbourhood, and many attempts were made to shoot it, but so completely had it adopted the habits of its paternal relations that it would suffer no one to approach it within gunshot, and yet this duck was only half of the wild breed."

Here we certainly see that very slight changes of condition can do much towards determining whether an earlier instinct or a later habit become quite hereditary, will prevail. My great question is, whether man's unconscious influence in ministering such marked yet unnoticed and unrecognised changes may not be in some way as great as his intentional artificial selection justifying a new classification and a new scheme—unintentional selection?

XI.

Colonel Butler had a query in the "*Zoologist*" [or "Ibis"] some years ago, respecting the power of the woodpecker to see in the dark. His query arose in this way: he found a woodpecker's nest with but one egg. Cutting a round circular piece out of the tree just below the nest, he was able to extract the true egg, and to put in its place a thrush's egg about the same size and shape, though very different in colour. Having done this, he at once filled up the hole with the bung as near as he could, exactly colouring it over to a likeness to the bark of the tree. Almost to his surprise after all this, he found that the woodpecker stuck to the nest, and when she had laid four more eggs, he took out the bung to find, to his surprise, also, that the thrush's egg had been rolled into the recess left by the bung not penetrating far enough in to get even surface inside as well as out, and the thrush's egg almost fell out when he extracted the bung. Now it was clear that when the bird was in the nest the place was quite dark beneath her, and how did she know that the thrush's egg was not hers, which she most conclusively showed that she did. But have birds no sense either of touch or of smell?

Either of these senses might have aided the bird, even if no light was there.

The coot will not sit upon duck's eggs. The sense of smell in certain birds must be very keen; for a gentleman, a friend of mine and true naturalist, in Essex, has persevered with experiments with the coot in this direction; he had at last the eggs of the mallard and teal carefully touched up to imitate the coots' eggs, and taking away the coots' these eggs were carefully substituted; but the wary coots were not to be done. They abandoned the nests and at once set about laboriously building others at different points round the pond, and nothing would tempt them back again, though their own eggs were restored. They would not be tempted even so much as to look at the nests. The argument suggested by these facts is exactly on all fours with that from the woodpecker and the thrush's egg.

And one question which arises here is important, radical. If the woodpecker, *in the dark*, is able so decisively to detect the egg of a thrush, about the same form and size, as not its own, why is it that so many birds fail to discriminate, and in their open nests, with full light to aid them, between a cuckoo's egg, which is much bigger frequently, and almost always a little bigger, than their own eggs, and to adopt it and hatch it, and, at great labour, rear the alien nestling, to the destruction of their own brood —sometimes to their own destruction—their instinct or intelligence just there failing to protect the species.

Is the one instinct a "misleading instinct," and the other "lack of instinct"? or what is it, or which? They cannot both spring from the same source. Besides all which the little birds, many of them, must have had experience, and have utterly failed to profit by it; while the woodpecker, with no experience at all, rejects an egg which, to certain senses, must in the dark seem very much like its own, or the coot rejecting the thrush's egg and deserting the nest.

In one word, Nature has here armed the woodpecker and the coot with a wonderful instinct against brooding alien eggs—an instinct, by the way, which is seldom or never called into exercise; while Nature has failed to arm many small birds with any such instinct, as regards the cuckoo's eggs. Why is this? Mr. Romanes argued with all his might that the deposition of cuckoos' eggs was comparatively so exceedingly rare an event that Nature had not deemed it worth her while to call out a counteracting instinct,[15] mark you; but here is a puzzle which has two branches:—(1) She has apparently armed the woodpecker and coot with this instinct, without any great call to do so, deposition of alien eggs in her nest being certainly an exceedingly rare event; and has not armed many small birds, where there is assuredly the greatest call; for (2) the deposition of cuckoos' eggs is not rare either here or in foreign countries—

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[15] "Animal Intelligence," pp. 308-310.

whole species being much reduced on this very account; so that Mr. Romanes was either writing in ignorance or writing so from design, helplessly. The problem remains; why has Nature bestowed on certain birds so strong an instinct, which is seldom or never called into use, and refrained from bestowing it where, for preservation and increase even of the species, it was much needed; and (3) how is it that a few species of birds more and more in all countries have come, and are coming, to reject or build over the cuckoo's egg?

Nature seems, from the Romanes point of view, to be so very capricious that she has no law at all; she arms birds with a strong instinct where practically they do not need it—the deposition of a thrush's egg in a woodpecker's nest being *not* "comparatively, but absolutely, an exceedingly rare event"; and where they do much need it—the deposition of a cuckoo's egg in the nests of other birds being, by comparison here, not at all an exceedingly rare event—she takes care to think it not worth while to arm them against it or to allow them to learn anything by constant experience of injury to the species—in a word, Nature is too like, alas! to Jahvè, the Hebrew god, according to Mr. Romanes; a playful bully and capricious tyrant, full of favouritisms and dislikes, allowing species to increase by wilfully depriving others of "counteracting instincts," which she does not think it worth while to develop or to call out, because it is "not worth her while." Mr. Romanes was a bit of a theologist, and here he is so, too, and did not quite know it.

When, therefore, Mr. Romanes said Nature, because of the rarity of deposition of cuckoos' eggs, had not thought it worth while to bring into play a counterbalancing instinct in the little birds, he was doubly wrong—wrong (1) as to the fact of depositions of cuckoos' eggs being comparatively an exceedingly rare event, and (2) as to the fact that Nature had not thought it worth while to call into play, in victimised birds, a counterbalancing instinct; and the question to be answered by Mr. Romanes' friends and followers now is, why Nature has called into play this counterbalancing instinct in *some* few species and not at all in many others. If they will satisfactorily answer that, then we shall heartily thank them; till then we shall say that, so far as they follow their master they but "make confusion worse confounded," by assuming certain things and then, on the ground of these false and most groundless and ignorant assumptions, speaking categorically for Nature in her doings or not doings. And the case against Mr. Darwin is here proved—the case which, he said, would, if found, overturn his whole theory—for I, for one, cannot see a single item of benefit which the instinct in the small birds, which leads them to accept and hatch a cuckoo's egg and sometimes to kill themselves in the endeavour to satisfy the little monster, is for the benefit of their species, in any case.

XII.

Another peculiar problem arises here suggested by many phenomena in the field of parasitism. "Instinct," in the ordinary sense, here over large areas seems to get absorbed in and wholly overcome by another instinct which sets the protecting or tolerating creature completely at a disadvantage. This view of the matter has, we confess, been suggested to us by the facts which we could not but attend to and endeavour to see in true relations when dealing with the cuckoo, where we find an ordinary instinct of protection of eggs and young completely overcome by another instinct of protection for the young cuckoo and the nursing and feeding and tending it. On these points I can only refer the reader to a volume on "Our Common Cuckoo and other Cuckoos and all other Parasitic Birds." The facts arising from the study of the cuckoo led, however, to an investigation of parasitism in the natural world generally; and certainly, in not a few cases, the facts are not at all explainable save on the assumption that all ordinary instinct just there ceases to assert itself, and another and wholly different instinct emerges and is supreme, and this often, so far as we can see, to the detriment alike of the individual and of the species that entertains and protects, or passively tolerates the parasite.

In fact, the whole realm of parasitism, alike in flowers and plants, in insects and in birds, presents an almost unexceptional argument against Darwinism. We read :—

"The social Hymenoptera—ants, bees, wasps—well provided with weapons as they are, neither have the sense to exterminate their enemies, nor do they seem even to recognise them. In the presence of their habitual parasites these insects, in other respects so sagacious, seem to be struck blind."[16] Surely, here we have an instinct which actually becomes protective, which is in favour absolutely of the protected species, and has no benefit or compensation from the protected for those which protect.

There is the *Sacculina* and the crab, which by the presence of the *Sacculina*, whether male or female, is rendered sterile—utterly sterile. In the female, where the tail is segmented and flexible, that tail which had hitherto protected its own eggs now protects only the *Sacculina*, and in the male the tail (which is normally segmented without being flexible) becomes exactly like that of the female. No man will surely deny that here is a case where an accomodating instinct in the preyed upon becomes absolutely of benefit to the parasitic species, and of loss and injury only *both* to the individual and to the species protecting.

Here are structures which become essentially modified in favour of an alien parasitic species, and one effect of that change of structure is sterility in both male and female, whose instinct of protection has been assenting and is a cause. In other cases, as well in plants as in animals, there are self-adaptations to the entertainment of the parasite.

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[16] Massart, p. 71.

There are certain blind beetles (*Staphylinidæ*) found in ant-hills, living at the expense of the ant community, and, so far as can be ascertained, yield no service or benefit to the ants in return. The instinct of the ants which tolerates the beetles is surely an instinct entirely in favour of the beetles, and of no service to the ants. "Although the fact is not yet proved, it is, at any rate, very likely that at an earlier period there had subsisted between these species and the ants some kind of mutualist relation analogous to that which still exists between ants and aphides. Little by little this relation has ceased, until the animals found in ant-hills to-day have become traditional parasites, tolerated through the inertia of custom, on account of the services formerly rendered by their ancestors."

But, with all respect to the author, this is a mere speculation—a maybe, while the facts presented to-day are as we have stated them. Mr. Darwin deliberately says in his essay on "Instinct," published by Mr. Romanes in his "Mental Evolution in Animals," an appendix:—

"Mistakes of instinct may in some cases do injury to a species and profit another; one species may be compelled, or even apparently induced by persuasion, to yield up its food or its secretion to another species, but that any animal has been specially endowed with an instinct leading to its own destruction or harm I cannot believe without better evidence than has hitherto been adduced." (P. 377.)

Is, then, the whole behaviour of the small birds victimised by the cuckoo a mistaken instinct, or what is it? It leads to the destruction of larger numbers of certain species and to the inevitable weakening and utter incapacitation for breeding of the species victimised!

XIII.

The vast field of mimicry, too, suggests many difficulties.

Prof. H. O. Forbes has this passage on the wonderful spider, *Thomisus decipiens*:—

"I observed one of the *Hesperides* at rest on a leaf on a birds' dropping. I had often seen Small Blues at rest on similar spots on the ground, and often wondered what the members of such a refined and beautifully-painted family as the *Lycænidæ*, could find to enjoy as food seemingly so incongruous for a butterfly. I approached with gentle steps but ready net, to see if possible how the present species was engaged. It permitted me to get quite close, and even to seize it between my fingers; to my surprise, however, part of the body remained behind, and in adhering, as I thought, to the excreta, it recalled to my mind an observation of Mr. Wallace's on certain coleoptera falling a prey to their inexperience by boring in the barks of trees in whose exuding gum they became unwittingly entombed. I looked closely, and finally touched with the tip of my finger the excreta, to find if it were glutinous. To my delighted astonishment I found that my eyes had been most perfectly deceived, and that the 'excreta' was a most artfully coloured spider lying on its back with its feet crossed over and closely pressed to its body. This species does not weave a web of the ordinary kind, but constructs, on the surface of some prominent dark green leaf, an irregular-shaped film of the finest texture, drawn out towards the sloping margin of the leaf into a narrow streak with a slightly thickened termination. The spider then takes its place on its back on the irregular patch I have described, holding itself in position by several strong spines on the upper sides of the thighs of its anterior pair of legs, thrust under the film and across its legs over its thorax. Thus resting with its white abdomen and black legs as the central dark portions of the excreta, surrounded by its thin web-like film, representing the marginal watery portion become dry, even to some of its trickling off and arrested in a thickened extremity such as an evaporated drop would leave, it waits with confidence for its prey—a living bait so artfully contrived as to deceive a pair of human eyes even intently examining it."

The Rev. O. P. Cambridge, M.A., C.M.Z.L., who rendered Professor Forbes aid in the identifying and classifying of certain of his specimens, was inclined to find in these most remarkable facts merely an illustration of natural selection. He asked, "Is not its exactness probably the result of the unconsciousness of the spider? Conscious design would possibly have resulted in failure and abandoning the plan, or at least a more clumsy imitation?" Professor Forbes only reiterated his former impressions that "the similitude is so exact that the spider might have had consciousness, and it could not have been more exact if the spider did have it."

Under many definitions of instinct we have, according to some writers, conscious procedure or action, as regards the steps in a process, but without any consciousness of the end aimed at or to be attained by them. Professor Cambridge would here actually go the whole hog, and reverse all this of Mr. Romanes and so many others. He would make the exactness of the imitation a direct result of unconsciousness (which, by the way, only carries the difficulty one step further back; for consciousness of the steps of the process, as well as the result of the combined machinery, must have been realised in the origin of it somewhere, else you have a mere chaos, lawless and contradictory altogether), while of the end the spider must have become conscious, as he secured and ate the prize—that is, he was conscious of the end or aim (against von Hartmann, Romanes, and the rest), though the end was greater than the means, and included them. Von Hartmann should know of that, and work it out. But as to end and consciousness of end, let us ask this question:—Does this spider, when it awakens to a fine tit-bit secured by this admirable mimicry and device to secure the end, awaken to consciousness of the end being secured, or does he not? and does he derive from such consciousness any stimulant to further efforts? Does he eat without consciousness of eating, and feel the labourer is worthy of his hire, or does he not? Is the spider in this case any different from the bit of straw that chances to be blown into a spider's web, and looks, glittering there, very like something else? and, if he is, then will Mr. Cambridge kindly tell us the exact point at which consciousness returning to the spider makes him so?—he is different from the bit of straw, being animate, with a belly to be filled, if not with consciousness, and that, don't you see, makes him quite different from the bit of straw, to begin with. You might just as well say that the fisherman, who by long practice, added to some measure of inherited tact, when he mechanically makes or mends his nets is unconscious, and does his work the better for unconsciousness, and only becomes conscious when he eats a portion of the haul he has made by aid of the said nets. Or that I who, because of long practice, am by no means conscious of definite effort in the forming of each of these letters and words with the pen I now hold, am not conscious, and write all the better because of my unconsciousness, and only awaken to it when I get the little money for my effort. The little money is an end, but I deny the unconsciousness as a general element in every effort I make, though certain exertions of nerves and muscles essential to the process are not raised into the region of special consciousness. "Maybe's," "possibles," "probables," are the magic catch-words of these fellows, and used absolutely often without sense and without regard to the full effect of what is implied logically in them. An act of reason must —at least in some individual mind must—have made or suggested at least the first step in this wonderful and almost artistic mimicry.

When we read such passages as the following, very much the same problems are presented to us :—

"The females of the *Meloë*" (that is one of the oilbeetles of which Cantharidæ are typical) "lay their eggs near the ranunculus and other plants, whose flowers are regularly visited by bees. After these are hatched, the larvæ ascend into the flowers and wait patiently till a bee takes them on his back and carries them into the interior of the hive. This insect was formerly known as the bee-louse, but this appellation is incorrect. The bee is not the host of the Meloë, but simply its beast of burden. According to recent observations, flies perform the same office for *Cheléfers*, and certain aquatic and land Coleoptera for several kinds of *Acaridæ*."

The problem of instinct, more especially as an inherited gift, is here complicated by considerations of a novel kind. How can the mature insect communicate any kind of habit, however beneficial, to the species, to an egg, then to a larvæ, and then to the mature animal. The instinct in the larvæ of Meloë is just as perfect as regards the ends aimed at as in the perfect insect, if not indeed more so.

XIV.

The simple truth is Mr. Darwin, by his efforts to make a term or two cover the secret of all life has set a bad example. He was an indefatigable observer; a wonderful collecter of instances, with a certain power of putting them together, and from his own point of view, drawing inferences from them, helped out with many "maybe's," "must be's," "probably it was'es," "apt to be's," &c., &c. But he was not a clear thinker. His mind was, after all, not an acute or an incisive mind: he pondered much, but not seldom confused an "end" with an "aim," or a "motive," and then went on persistently arguing as though they were the same, which they are not, and never can be, a point where logic is justified of her children. All this is seen with especial clearness, in his way of dealing with the word *instinct*, which, despite his able gathering of facts, and, in a way, very effective presentation of them, he renders, to one who thinks and will not be imposed on by mere *authority*, only confusing—"confusion worse confounded" indeed. In nothing was this more seen than in his eighth chapter of "Origin of Species," which should have been one of the strongest, and yet is one of the very weakest, washiest and most unsatisfactory, perhaps, that a great scientific man ever wrote. He is wrong in many of his facts as regards cuckoos, even as I myself have observed and tested, and as I have tried to set forth elsewhere; and certainly he was far wrong in his inference that the parasitic impulse of *Cuculus canorus* (our common cuckoo), was due in any way to short time here, determined by date of migration; whereas a very slight comparative survey shows that *Cuculus canorus* itself in other climates is either from six to eight months present in the breeding-place, and in others is resident and is still there when the young ones are mature, while various other cuckoos are actually and unvaryingly resident, as for example, the koel of India, and the great spotted cuckoo in Egypt, as well as others in Australia, New Zealand, &c., and yet they are as confirmedly parasitic as our common cuckoo is with us. Even so long ago as 1848, Mr. Blyth, in the *Asiatic Society's Journal*, pointed to some of these facts as raising doubts about Jenner's generalisation, which Mr. Darwin, alas, too slavishly adopts and swallows, and later, in 1862, Mr. Jerdon, in his "Birds of India," wrote:—

"That their migratory habits, as suggested by Jenner, have anything to do with it [their parasitism] is contradicted by the fact of the existence of many non-migratory cuckoos (the common Indian koel, for example), being equally parasitic." [17]

And yet Mr. Darwin went on issuing, so far as I know, his eighth chapter of "Origin of Species," without any radical change, any recognition whatever of the vast fields of fact opened up by Mr. Jerdon's suggestions of 1862, in book form and in scientific Indian journals of years earlier, which it was plainly Mr. Darwin's duty to have seen and read.

But Mr. Darwin preferred to hang himself simply on to the coat-tails of Jenner, or to crawl up and perch himself on that worthy's shoulders, so as to get a new view of the world, arguing out helplessly to sustain Jenner's ideas, in which business Mr. Romanes, in his "Animal Intelligence" and "Mental Evolution in Animals," so far follows suit just as helplessly; and instead of attempting the slightest comparative survey, which would have helped them to correct their mistakes, notions and courses, go on persistently to delude themselves and to delude others—letting Jenner, of more than a hundred-years-ago-date, really do their thinking for them, and, like gin-horses, both of them persistently decline to be moved out of that hundred-year-ago beaten round! Is that worthy of the two great evolutionist thinkers of the nineteenth century? Evolutionist *thinkers*, indeed; say rather blind followers, these, of old-fashioned, too speculative Jenner, who would not be content with his facts, but must frame a theory into which they must be made to fit; and in 1859 and in 1887, here are the two great evolutionist thinkers of Old England, refurbishing up Jenner and exhausting ingenuity at the wrong end to fortify what he said, in spite of all warnings of more practical scientific men.

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[17] "Birds of India," i., p. 321.

Much, very much, might be added in the same direction, to show that origin has a great deal to do in discriminating "instincts," as so named by Mr. Darwin from other instincts, which are so named by him also, but which are qualified and distinguished from the first class by influences operating effectively to produce the powers, whatever they are, from what operated to produce the others—a distinction which is not properly defined and presented, surely, by mere vague adjectives, put in front of the word "instinct," such as "original," "extraordinary," "strange," "cruel," "misleading," "mistaken," &c., &c., which surely, in a strictly scientific sense, define nothing, and are themselves misleading.

We cannot now go into minute criticisms of definitions of "instinct," nor would it prove profitable, but we draw one moral from Mr. Darwin's notable looseness and confusion here, namely, that it would be well if we could get rid of the word "instinct" altogether, and find other and more definite terms for what we mean when, like Mr. Darwin in this, if unlike him in other things, we use this word and are tempted to stick unmeaning adjectives in front of it.

Just as we pass this proof for press, we read Professor A. Sedgwick's remarkably clear and effective Introductory Address to the Zoological Section of the British Association at Dover, 1899. In the Appendix we present confirmation from an unexpected point through him as regards the play of conditions that inevitably comes into force in the very fact of sexual copulation, determining so much else even in the progeny—a thing utterly unknown in the realm of asexual generation. Here we are led to make note of this address because of a very admirable definition of instinct which he there presents, which also gives a good scientific reason for our slowness to comprehend its swiftness of action. This definition is inclusive, where so many are exclusive, setting outside the circle some of the very facts that press for explanation. This is what Professor Sedgwick said :

"An instinct is nothing more than an internal mechanism which is developed with great rapidity in response to an appropriate stimulus. It is difficult for us to understand instincts, because with us all developmental processes are extremely slow and gradual. This particularly applies to the development of those nervous mechanisms, the working of which we call reason."

PART VI.

MR. WOLLASTON HUTTON ON DARWINISM AND LAMARCKISM.

Since this book was written and in type, Mr. F. Wollaston Hutton—an enthusiastic follower of Darwin—has published a volume—“ Darwinism and Lamarckism Old and New ”—the main aim of which is to justify Darwin against certain more recent developments, especially the criticisms direct and indirect of Mr. Herbert Spencer. Mr. Hutton has for many years been resident in New Zealand, and can bring not a few very effective instances in proof of his theories from the “ changes of condition ” on creatures taken to New Zealand from the British islands and other places, the fresh-water trout, for example, there beginning, like salmon to affect salt water, and is very greatly modified by this. The book is made up of four lectures, delivered at different times to learned societies in Canterbury, New Zealand, and they certainly were privileged to be presented with so much that is thoughtful and suggestive. But, as regards the general thesis, we are on the other side of Jordan from Mr. Hutton, and though we have read his book with great interest and pleasure, admiring his large knowledge and resource, we certainly cannot on the central positions bring ourselves to agree with him.

I.

Mr. Hutton, in dealing with what he calls the Neo-Lamarckian School, of which he sets down Mr. Herbert Spencer as a leading exponent, has this sentence :—

"They argue that progress has been mainly secured by the inheritance of post-natal variations which have arisen either through the attempts of animals and plants to adapt themselves to external conditions, or by the direct action of external causes in the individuals." Darwin, he says, held that "post-natal variations, due to direct action of external conditions, were seldom, if ever, transmitted."

Even though we were to admit that this was a correct representation of Mr. Darwin's later views, as it is of his views in the "Origin of Species," we yet may offer some remarks on the principle itself. Mr. Hutton gives us in summary Lamarck's deliverance about ducks—that they were not made web-footed, but became web-footed by effort in the water, so that by constant stretching of the membranes of the toes, in persistent efforts to swim, the membrane between them became extended and extended, till now they are as we see them. The same argument would, of course, apply to all the web-footed water birds, the process in an early stage being now seen in the coot. The modification on the feet of all water birds by webbing is according to this nothing but an accumulated result of post-natal variations on whole species of birds.

But Mr. Darwin himself set forth an exactly analogous case in the wasp with a "mental peculiarity" which *happening* (that word proves its post-natal individuality) "to sting a larvæ wherewith to feed its own larvæ in the nervous centre, discovered it could paralyse without killing," and that there was benefit for the species in it. The wasp "with a mental peculiarity" thus on the whole theory, developed post-natal powers or variations at first purely individual, which somehow gave rise to a change in the whole species, modifying and developing the stinging weapon of the wasp, and through it of much else; so that here, just as in the case of the ducks, you had on this supposition of Mr. Darwin's own precisely what the neo-Lamarckians claim — post-natal variations with hereditary force.

Then there are points involved in Sir John Sebright's instance of cross between tame duck and wild duck at p. 254 *supra*, which raises further complications and difficulties.

And here is a very peculiar case. The young grouse on Yorkshire and on some Scottish moors, quite as decidedly as the old ones, at once abandon the usual line of flight when shooting begins, and fly low—so low that their hues mingle with those of the heather, and they are with difficulty seen; only the most expert shots being able to touch them. The question rises here, was that a post-natal individual attainment or modification of habit at first, or was it not? and a second question follows on that: has it become transmissible and hereditable, or not? Answers to these questions will oblige, and, however answered, some further applications to Darwinian principles will arise, (1) as regards origin of post-natal experiences leading to variations, and (2) as regards transmission of these to progeny.

At some point, on Mr. Darwin's own clearly expressed theory, one cuckoo began, instead of brooding its eggs, to put them in the nests of other birds; as Mr. Darwin would say, began thus the formation of "an occasional habit," and, as he says, so it actually is now with the two commoner American cuckoos—an "occasional habit," which must very speedily have effected other habits and even structure. Surely this was at first a post-natal variation, due to some change of conditions which we may be able to trace, or may not, and neither Mr. Darwin nor any one else has yet accounted at all, not to say satisfactorily, for its passing to a whole species, and even to species nearly related to it. The neo-Darwinians, with Mr. Romanes at their head, are just as far out in trying to explain or account for this as Mr. Darwin himself was; and as the cuckoo was treated by Mr. Darwin in the eighth chapter of "Origin of Species"; and the whole thing accounted for by shortness of time in the British Islands, owing to migratory impulse; and as migratory impulse is always directly dependent on, and determined by, conditions, we really have, you see, a general as well as a particular confession from Mr. Darwin himself of the inadequacy or falseness of his own theory, and the truth of that, so far at all events, of the neo-Lamarckians. There is no getting away from this, which exactly illustrates the truth of the remark of another, that Mr. Darwin found the cuckoo "about the most difficult subject he had to tackle." Such things as this, lying even in the "Origin of Species," show that Mr. Darwin himself was and remained quite unconscious of some of the links that, *pace* his confessions of error, do yet bind his earlier works to his later ones, as also of certain things, besides those acknowledged, that definitely separate them.

Mr. Hutton does not and will not fully recognise the great changes of front Mr. Darwin effected—the full extent of which, indeed, he dextrously, and in so far successfully, tried to hide. This is a common weakness of almost all Darwinians; and Mr. Wollaston Hutton certainly, if he sins, sins in good company. Mr. Darwin, to the last, went for great slowness—to such an extent really that he would not recognise any effect whatever from confessedly operative *causes* for several generations; an effect "not appreciable to *ordinary* observation," being a refinement quite beyond him, though, somewhat inconsistently he was, as Mr. Hutton reminds us, very apt at trying to fortify his theory of species by stating that, if not demonstrated, it explained so many other things, and used, as he says, to cite the admitted theory of undulation of light.

"I am actually weary of telling people," he wrote to Mr. Hutton in 1861, "that I do not pretend to adduce direct evidence of one species changing into another" [Saturnia and Metzger's maizes, &c., &c., not yet turned up, you see, to aid him here had he wanted such, which he did not;] "but that I believe that this view in the main is correct, because so many phenomena can be thus grouped together and explained." [Saturnia and Metzger's maizes too?] "But it is generally of no use; I cannot make persons see this. I generally throw in their teeth the universally admitted theory of the undulations of light—neither the undulations nor the very existence of either being proved—yet admitted because the view explains so much."

"Methods of Isolation" is nothing but a phrase, invented by Mr. Romanes to cover in so far Mr. Darwin's utter failure to perceive, or if he perceived, clearly, fully and frankly to admit the seriousness of his error in his "Origin of Species," in practically denying the force of conditions as determining variation and determining it in many cases very quickly, so that we have these really most astonishing results in Darwinians: (1) the eyes closed to the *real* bearing of no end of things in "Origin of Species," of which Mr. Darwin himself confessed his renunciation as well as (2) some of which he was not fully conscious; and (3) the endeavour always to minimise the great gulf fixed between the earlier conscious teachings and the later, as though there was no vital error or contradiction (as Mr. Darwin himself clearly confessed there was), but that the later teaching was only a natural development, continuation and extension of the earlier instead of being directly opposed to it. Mr. Darwin unwillingly, was forced more and more to slide along the line landing him in true Lamarckism; and to us there is more of irony than enlightenment in some remarks of Mr. Hutton, to the effect that, after all, there is not so diametric an opposition between them as at first sight appears. Of course not. The "Descent of Man" and "Domestication of Plants and Animals" indicate movements—only half acknowledged it may be—towards the Lamarckian plane; for, by this time, as said already, Mr. Darwin had not only awakened to the influence—the vital and determining influence of conditions; but to the direct influence of conditions on the most sensitive of all organs—the sexual organs—at first strictly post-natal, you see—which certainly he had not, and was much inclined to lessen and ignore or set aside in the "Origin of Species" the very slightest changes, as Mr. Darwin confessed, often materially affecting and modifying these.

Neither Mr. Romanes nor Mr. Hutton will in the least allow that this sliding towards Lamarckism was a turning of his back on his "Origin of Species" and its central doctrines, and therefore the reconciliations they try to effect between first teaching and last teaching are exactly like Mr. Benjamin Kidd's valiant effort to demonstrate that Weismann, like Scotland in Macbeth, stands exactly where he did. But that can have influence only with the people for whose suffrages the true philosopher or man of science would not care an iota; for they are the people who do not and cannot think—the people who swallow what they read from those who have gained *authority* by the false and bad process of booming or of being boomed, and who can no more compare, analyse and think for themselves than can the pump-handle. And more than this, till the Darwinians once for all grasp decidedly the very great contradictions between the most decisive essential positions in "Origin of Species" and those in "The Descent of Man" and "Domestication of Animals and Plants," they are, as it were, paralysed, and no real scientific progress can by them be any further made; because there is a confusion which they strive to keep as a confusion, and argue that it is a consistency, and throw a veil over it; and, so trying to do the impossible, they waste their energies—fain to walk both ways at once—backwards and forwards, and thus really stand still and make no progress. Nor will they really make any till they set clearly before them and confess that Darwin's errors, *which he acknowledged*, went deeper even than he thought, and that the "Origin of Species" must simply be thrown out as immature, inadequate, partial, unfounded, contradictory, inconsistent—a book only for scientific men, by which to trace the progress of Mr. Darwin's mind, but with no true and positive basis, unless a bad, erroneous and misleading basis, as Darwin himself, in his "Life and Letters" really confessed, as we have seen, quoting his own words at p. 10.

II.

Mr. Hutton lays it down as a true interpretation of a great principle of Darwinism that post-natally acquired mental characters can be transmitted, but not necessarily post-natal structural characters; even those mental characters, that come by inheritance, can only occasionally be transmitted after several generations, *i.e.*, pass from the indefinite to the definite stage; but structural variations do not pass. Now, here, as is often the case with all these thinkers, a great distinction is made between two things as though they were in separate chains of causation, whereas they are not, but the one rather related to the other as effect to cause. I lay it down as an absolute law that a mental character operating in any manner effectively is from the moment it does so producing, whether we can trace it directly or not, modifications more or less subtle on structure. It must modify brain or nervous system, and through it operate on structure more or less. The *Megapodii* have immense feet compared with other birds for their size; these feet, we are justified in saying, have been so developed by a mental character inherited which directs them to hatch eggs by heat of fermenting vegetable matter in heaps of soil which they scratch together to form mounds. This forming of mounds and dropping vegetable matter into them must have had a beginning—the mental character there took expression. From the moment they so began to act the feet began to grow bigger from the yearly exercise of certain muscles not so much used before, although for long it might not have been noticeable to the ordinary observer, but it was an essential part of a process which had for one cause a mental character, and that process going on for generations made the feet noticeably bigger. If there was transmission of mental character, it cannot be separated from the necessary effect of its operation producing the big feet from which the group take their generic name. So of Mr. Darwin's wasp with "mental peculiarity"; so with the cuckoo and the mental character which led it to drop its eggs into other birds' nests, and to modify its structure by non-brooding and polyandry, the ovary being able to retain perfect eggs much longer than that of other birds can. When Mr. Hutton quotes Sir Samuel Baker to the effect that African elephants invariably use one tusk in preference to the other, and yet that this has produced no difference in size, that, indeed, the used tusk is smaller than the other because of wear and tear, he gives an instance weakening to Darwinism, for according to it the tusk in use should be larger and the unused tusk smaller. In deers' antlers, where the law of symmetrical growth is not exhibited, the Darwinians have the same difficulty fronting them, the more there that the least disparity in the antlers indicates modification also of the sexual organs, which Mr. Darwin at last came to see were the most sensitive of any part of an organism.

Here, again, we are forced to perceive the contact of Weismannism and Darwinism. Darwin at first held (in "Origin of Species" which, despite the master's clear renunciation of leading positions in it, Mr. Hutton still practically swears by) that variation was practically independent of conditions, of external agencies, and depended upon constitutional variations; his phrases, definite and indefinite variations, emphasise his contentions — definite is that which belongs to the species and will be transmitted; indefinite is of the individual, and may, most probably will, die out with the individual—so that, here we are again, led by Mr. Hutton back on what is clearly, as Mr. Romanes would call it, a realm of chance, as Mr. Grant Allen calls it "the rough and ready fashion," figured in his chances of survival. There is no escape—none—for Mr. Darwin and his followers here—unless they are prepared to tell us that he renounced his confession set down in the words quoted above from "Life and Letters" (pp. 10 and 17). He was often renouncing—often modifying, often recalling, correcting, and too often trying in vain to hide the full effect of it—perhaps he did so here, though really we are unaware of it. But if, with this utterance full in view, Mr. Hutton holds to Darwin's consistency as to selection, if it is to continue to bear any true meaning; then we say that Mr. Darwin was quite right and that "Natural

Preservation" would have suited him far better, since there can be no doubt certain things and beings have been preserved, but legitimate doubts may well arise in many cases about their having been selected or favoured, as he elsewhere naïvely puts it—for a variation—as we have seen. "Fitness," Professor Cope says "is of various kinds and can hardly be regarded as simple." Mr. Darwin always wished to regard it as simple, and this was another source of his error.

Weismann vainly tried to demonstrate that the environment acted on the protozoa only—he assumed that the action of environment was small and only acted in the most indirect manner, if at all, on the reproductive cells. But, as we have seen, facts forced him to surrender that, and to admit that no more than a chosen, very small, nucleus of his own remained undirectly affected even in the germ-cells. Sachs has made the matter clear enough in its applications to plants. He says, and certainly his words do not favour Darwin or Weismann:

"In reproduction, we must assume that certain particles of matter pass over into the reproductive cells, which have been previously produced and their nature determined by the organs of the mother-plant, and now possess the capability, whilst appropriating to themselves new matter from without, of imprinting upon this the same series of material differences as had already declared themselves in the mother-plant. It is this process which is termed heredity, but heredity is often treated in a very thoughtless manner as a force of nature by which it is imagined to be possible to explain all manner of things."[1]

We cannot seriously regard it as any part of our business here to present alternative theories to those we criticise; but we may interject at this point one suggestion. What if the characters which are admitted to *lie dormant for generations* in, say, fancy breeds of pigeons are thus transmitted from generation to generation and rendered of none effect or overcome by man's devices; but the moment these are withdrawn, they assert their presence and force, for it is a fact, that any two breeds crossed will give the original blue lines and other marks of the blue rock pigeon and, if let alone, the progeny will become again pure blue rocks. This seems to us to be borne out also by the results of Professor Cossar Ewart's experiments with horses and zebras and their progeny, as dealt with already (see p. 151). Here is one intelligent biologic law of what is called reversion. "Characters can be suddenly acquired," says Mr. Hutton, "but they cannot be suddenly transmitted" (p. 166), and yet there is the case of Artemia to be cited immediately and the case of axolotl and amblystoma, which will persist if conditions are only maintained.

In face of the master's own admissions, even of serious error on this very point, Mr. Hutton sets it down that "evidently changes of condition will not account for variation," and that "individual constitution is of more importance." Surely this is too bold and absolute in face of what we have quoted from Mr. Darwin—that is to say, it returns on the "Origin of Species," and practically repudiates the later deliverences of "Domestication," and "The Descent of Man." Both cannot be right nor can there be reconciliation—the one is opposed to the other. Mr. Hutton boldly and unqualifiedly says "that one species cannot give rise to two without the help of isolation."

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[1] "Physiology of Plants," p. 8.

Professor Cope says:—

"I have changed the spiny *Ononis spinosa*, the Rest-Harrow, both by cuttings and by seed into a spineless form, undistinguishable from the species *O. repens* in two years; but it would have, I doubt not, at once reverted to the *O. spinosa* if I had replanted it on the poor and arid soil from which I took it. ("Factors in Evolution," p. 227.)

And Professor Henslow says: "The spines originate, I maintain, as a mere accidental and inevitable result of the arrest of certain organs, such arrest being mainly due to drought."

Mr. Hutton himself tells us that the veronicas of New Zealand are very variable, while at home with us they are very stable.

Would Mr. Hutton consider that Dr. Ray Lankester's fish or arthropods which were swept into a cave by a cataclysm were modified by help of "isolation," or by change of condition, or what? If isolation, in his sense of it, is indispensable to selection, then clearly certain aspects of the specific meaning originally attached to it, have vanished out of sight, and we have only special pleading on an ingenious but an easy after-thought.

"The change of *Artemia* into *Branchipus*," Mr. Hutton writes, "is most extraordinary; for the latter genus, in addition to other characters, has an additional abdominal segment. How the absence or presence of salt in the water could determine this, the presence or absence of an abdominal segment is incomprehensible."

To a man who sets it down absolutely that "evidently changes of condition will not account for variation," and that "individual constitution is of more importance," this phenomenon, due, so far as can be discovered, entirely to the presence of a certain proportion of salt, may well be incomprehensible, though in less pronounced ways the same process is going on constantly and everywhere, summed up beautifully in the sentence from Buffon already quoted at p. 11. "Many species are being perfected or degenerated by the changes in land and sea"—modifying, for one thing, say, the proportions of salt present in the air and in the food of vast classes of creatures—"*ceaselessly proceeding*, by favours or disfavours of nature, by food or elements of food, by continuous influences of climate or changes of climate."

Professor Cope cites Mr. Darwin as admitting the insufficiency of natural selection as an explanation of the origin of certain characters; for he says (p. 119), "Natural selection, it should never be forgotten, can act solely for the advantage of each being." He goes further and admits (p. 114) that the Lamarckian doctrine has some claims to credence, where he says, "On the whole we may conclude that habit, or use and disuse have, in some cases, played a considerable part in the modification of the constitution and structure, but that the effects have often been largely combined with, and sometimes overmastered by, the natural selection of *innate* variations."

"*Innate* variations!" Well, we know roughly what Mr. Darwin means, in a kind of way, but he cannot be congratulated on nicety in the use of words.

The presence or absence of salt determines many things. In this case, we repeat, we simply have a very *striking* and immediate proof of what, in our idea, is going on constantly and more gradually, and that it is not perceived and not admitted. Mr. Darwin himself, to the very last, desiring to maintain a long period before any definite change could arise, even where it was plainly admitted that definite causes were operative—a denial of the truism that every cause must have an effect, if not analysed thoroughly to the foundation, a denial of the still more notorious axiom that every effect must have a cause—the clumsy, stupid *non-sequitur* and contradiction of Mr. Darwin that there was no effect for several generations, while he admitted the cause or causes was or were operative. If any other person were to write like this, they would be told very speedily that they wrote nonsense; but Darwin! The constant, if slow encroachments of sea on one coast, and recession at another, is clear enough evidence of changes constantly proceeding, likely to affect whole classes of creatures far beyond what is supposed.

III.

"In 1867" writes Mr. Hutton, "Mr. Darwin abandoned his former view on reading an article by Prof. Fleeming Jenkin in the *North British Review*, which convinced him that free intercrossing must necessarily destroy the effect of a single variation. He says that his mistake arose from thinking too much of the cases of selection by man. He then recognised that natural selection acts, not by preserving a few individuals, but by killing off all those which do not come up to a certain standard."[2]

So that even "Natural Preservation" which Mr. Darwin afterwards proposed instead of "Natural Selection" would not have been quite appropriate since it was not, according to this, a matter of preserving at all, not to speak of *selecting*, but of *killing off only*. "*Killing off!*" But then we return on the Master's own distinction. He most decidedly admitted, and the more thoroughly the longer he lived that this "killing off" was not always of the worst, but of the best and most promising. Question therefore arises, was selection—natural selection—alike exhibited in the killing off of the best as well as of the worst. If so, where is selection? Surely not justified in the killing off of the best—since selection there goes by a left-handed process of both doing and not doing. Either (1) the killing off of all those which do not come up to a certain standard is an assumption both as to the killing off, and as to the certain standard which is thereby directly made a very *un*certain standard, because (2) Mr. Darwin distinctly admitted that many, many of the very highest standard were killed off. These are his words as quoted already:

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[2] Hutton, p. 3.

"A vast number of eggs or seeds are annually devoured. . . . . Yet many of these eggs or seeds would, perhaps, if not destroyed, have yielded individuals better adapted to their conditions of life than any of those who HAPPENED to survive."[3]

Did Mr. Darwin anywhere, or in any form renounce the position here taken. If he did in order to make the "Killing off," according to Mr. Hutton, all clear, he only raised crowds of difficulties on the other side for himself.

With regard to "Killing off merely the unfit," the thing certainly cannot apply to the human species, if we may trust the recruiting reports in Great Britain. The vast percentage of men that enlist are *unfit*, a large proportion of those enlisted being rejected, or have systematically to be nursed up to *something like* fitness, so that, from the fit, and not the unfit, being killed off, we cannot keep up our little army by recruiting, and "conscription is in the air," in order that the powers that be may have a chance of drawing from the classes that are more favoured—I admit the propriety of the word *favoured here*—what they need.

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[3] "Origin of Species," p. 68.

Tolstoi says that it is the same in Black Russia—scarce a conscript fit to bear arms from that region, because he says over it the people have not had for years and years 70 per cent. of necessary food, amply proving, as respects human beings, what Dr. A. Russel Wallace had laid down regarding birds at one place. *Conditions* are surely potent here too—at all events, structural as well as mental characters are inherited, since it has come to the practical pass both there and here; and "statesmen" even of the pessimistic and "philosophic doubting" order, don't quite like the look of it, though they don't seem to see their way clearly to get to the causes, so deep and recondite are they so as to mend matters. "Things are out of joint, O cursed spite, &c." Certainly Mr. Darwin who was so fond of carrying the principles he fancied he had found illustrated in the lower creatures up into the human sphere, does not, at first sight at all events, *seem* to get support here, even though, as the somewhat too light and overwitty Professor Patten, Ph.D., of Pennsylvania, finds him the grandest latest interpreter of the non-moral Malthusian "idea." I do wish that great thinker and most profound statesman, Mr. Arthur James Balfour, would apply his fine and great mind to this problem during the recess and solve it for us—the Darwinian non-moral Malthusian idea reconciled with the moral and spiritual progress of the race—and comfort us with the idea that, though mental characters have been transmitted, structural have not, and exchange may yet be brought round by germs.

I cannot help thinking that it was rather maladroit in Mr. Hutton at all to refer to that incisive and powerful demonstration of the utter inconsistency and inconclusiveness of the "Origin of Species," by Professor Fleeming Jenkin in the *North British Review*, 1867. We have just refreshed ourselves by re-reading it. It is not a criticism, but a demonstration, which leaves Mr. Darwin stranded and helpless—not only on the point which he acknowledged, and his surrender of which led Mr. Hutton to make note of the article, but on all the other essential points—variability, correlation, lapse of time and so on. The Professor absolutely proves that "the rate of variation in a given direction is not constant; it is a constantly diminishing rate, tending therefore to a limit. New points are at first rapidly gained, but afterwards more slowly, while finally no further perceptible change can be effected." Which is right —Darwin with his universally obtaining slowness, or Jenkin with his fastness first and slowness more and more till a dead limit is reached? If this, indeed, is so, as Professor Fleeming Jenkin asserts, Mr. Darwin's doctrine in "Origin of Species" is without basis; Professor Fleeming Jenkin from other points demonstrates it, with the happiest use of mathematics and even engineering.

"The race is to be kept constant, to all appearance, for a very long while, but some subtle change due to time is to take place; so that, of two individuals just alike in every feature, but one born a few thousand years after the other, the first shall tend to produce relapsing offspring, the other shall not. This seems rather like the idea that keeping a bar of iron hot for a very long time would leave it permanently hot or cold at the end of the period when the heating or cooling agent was withdrawn." And from this and other circumstances, he lays it down, that the whole procedure of practical men, if Darwin is right, is in matters of breeding entirely wrong. "Breeders," says the Professor, "should reverse their tactics; instead of selecting the fastest colts and finest flowers to start with, they ought to begin with very ordinary beasts and species." I have heard, though I have no personal knowledge of the matter, that at Mr. Neave's establishment for breeding polo ponies this is at least partially done!—and that with the utmost success too.

He holds that "The Origin of Species" requires not the gradual improvement of animals, retaining the same habits and structure, but such modifications of those habits and structure as will actually lead to the appearance of new organs. But his argument in opposition to this is final:—"The tendency to produce offspring more like their superior parents than their inferior grandfathers could surely be of no advantage to any individual in the struggle for life."

Prof. Cossar Ewart is quite right when he says that inferences from his horse and zebra crossing experiments cannot be finally relied on for a long time; they cannot be fully relied on till we have reports as to the longevity, power of endurance, and stay under strain, &c., in these hybrids, till we can, in some measure, see from them whether or not the above is true, partially true, or not true at all.

IV.

Mr. Hutton makes a very wonderful extension of the idea of use, so extensive that anything could be made of it or done with it. He writes:—

> "We are quite accustomed now to the idea that every structure in a plant or in an animal has a special object, else it would not be there. But it is generally supposed that this object must be a useful one to its possessor, for that is the teaching of the old Darwinism. Now why should we limit ourselves to so narrow a view? We know that many structures exist which are not, *and never have been*, of use to their owner. Is it, therefore, necessary to believe that they are of no use at all? If we allow that the ultimate object of organic evolution

is the development of man, not only physically, but also mentally and morally, I do not see how we can escape from the conclusion that all these so-called useless structures, all that give us beauty and variety, have been specially designed for his education."[4]

Here Mr. Hutton, instead of supporting, simply abandons the whole Darwinian theory old or new, in favour of the old theological conception of things as created for the pleasure of man's eye and the pride of life, all being produced in prospect for the delight and gratification of man. If there are not special creatures, there clearly are things specially created or modified with a view to the pleasure of men's eyes and the pride of life in all human creatures. When the fertilisation of flowers was discovered, it was clearly seen that colour in flower petals was not *primarily* for beauty, but for use ; the wind could not see a colour or be attracted by it, and all anemophilus plants have practically colourless flowers. Flowers fertilised by day-insects are largely bright or deep-coloured, but those fertilised by night-flies are white or pale yellow to show well in the dark. So, also, with scents. Though it is true that the exposed ends of peacocks' feathers could not be so beautiful without beautiful colours even on the unexposed parts, we now know the reason that colouration is really set up by a chemical process in the pigment cells lying close to the blood veins of the epidermis, and that of necessity it will colour what is not seen as well as what is seen. But if Mr. Hutton's idea is to apply, it must apply both ways and all round. How about such "so-called useless structures" as are not beautiful, but ugly—approaching to deformity, or markings that are grotesque and certainly not beautiful or pleasant to the eye. Not to refer to other and more striking points, let us proceed to wind up this section with a sentence from Prof. Fleeming Jenkins' masterly article in *The North British Review*.

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[4] P. 107.

"It is difficult to suppose that every stripe on a zebra or quagga, or cross down a donkey's back, is useful to it. It seems possible, even probable, that these things are the unavoidable consequences of the elementary combination which will produce the quagga or a beast like it"—agreeing again, in essentials, with Sachs's and our theory given a page or two ago.

It is difficult to suppose that the formation of the coal measures had, in any respect, in view the taste, and scent, and love of new colours in men and women; yet the taste, and scent, and love of new colours in men and women have been gratified by saccharin and coal-tar scents, and aniline or coal-tar dyes or colours; so on the same principle, we might say that the flowers and tree-ferns, &c., which grew on this earth millions of years ago, were designed in order to gratify the taste, and scent, and eye of men and women of the 19th century, though the men and women of the 15th, 16th, 17th and 18th centuries, not to speak of earlier ones, might rather demur to the statement. Once allow this notion and we are back again with Paley and Butler.

The ugly, the unpleasant, the grotesque, or repulsive in these "so-called useless structures," what are they for? Not to speak of colour in human races, which no doubt is useful for protection in hot climates; yet persists most wonderfully, though families of such race have been settled in colder climates for centuries, where it cannot be for the same reason *useful*, we may mention prognathism, which, I am not aware, has been so absolutely proved useful physiologically. Yet Mr. Hutton ought to be able to base an argument on that too. Clear it is that if useful in tropical climates it cannot, for the same reason, be useful in temperate, and yet it persists, and ought to be included in Mr. Hutton's useless structures. If there was any such design there whole nations decline to see it or to acknowledge it. So that we are really in this position of regarding certain useless points of structure in plants and animals as designed from the first to minister to the pleasure of man; but certain points in the structure of certain races of men, for whom, mark you, all these others were designed to give pleasure, are not found to answer at all in the same way with other whole races of men. In fact, that argument really requires that there were but one race of human beings, and that a white race—preferably an Anglo-Saxon one—so that here some of the worst and most dangerous of race-prejudices would be brought into play and justified, and have new scope, as there is indeed some too-assertive traces of it just now in America—where, most certainly the negro race survives—and increases; but where a vast white population is almost unanimous by conduct in declaring that it is not a case of "survival of the fittest"; whereas the negroes and their friends may most justifiably assert that what "survives survives," and by the very fact is the fittest. The slave-traders and slave-holders, from their point of view, deemed the preservation of the negroes as survival of the fittest (for their labour), now the negroes are free, the case is not from this plane so very simple.

Mr. Hutton may well be called on to give his answer to these questions; he cannot have it both ways—and certainly both beauty and ugliness, grotesqueness and repulsiveness are represented there. Besides, what is one man's (or race's) meat is another man's (or race's) poison, and the assumption of some absolute standard of beauty or pleasure to the eye is wholly unwarranted.

Professor Cope points out that Mr. Darwin in many cases illustrates his unwillingness to look at disuse as the cause of conditions he describes. Under "Compensation and economy of growth," Mr. Darwin, he says, quotes from Goethe, that "in order to spend on one side, nature is forced to economise on the other side." Relative effects of use and disuse proceed contemporaneously, and it is probable that most cases of complementary growth-relations may be expressed in this way. Thus the orthognathism of the highest human races is accompanied by full frontal development, the two modifications constituting a retardation of the post-embryonic growth of the face. But this change can be traced to use, increased brain action enlarging that organ and expanding its osseous case, probably at the expense of lime salts, which would otherwise go to the jaws. Reduction of teeth in Artiodactyla and in man cannot be regarded as a useful character in itself, but it is complementary to the development of other characters which are useful."[5]

Prognathous jaws disappear from the higher races, but they do not wholly disappear from the negro races after centuries of residence in temperate climates, and where reasons of *use* alone can hardly apply; this is the more remarkable that brain action must be greatly increased, else in America the negroes could not succeed as they are doing in midst of competition and race opposition.

Much of this may, of course, be accounted for by their long period of servitude, slavery, when it was regarded as an offence against the criminal law to teach a negro to read or write; and when, though in another and more temperate climate, no impulse to brain action being given, the form of physical features would remain—brain-action being a more radical element to effect change of outward character here than change of external conditions, though external conditions, the brain-action once begun, would favour the outward modification and go hand-in-hand with climate, &c., in promoting it.

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[5] "Factors in Evolution," p. 248.

According to our principle, modifications must be more or less, though not very perceptibly, proceeding here; brain-action, to which, it is clear, the negroes are becoming now more and more inclined, must have its own effect; they are producing story writers like Paul Lawrence, painters, musical composers; and, were it possible, I should much like to have measurements of the heads of such members of the negro race, so that they might be compared with those of most nearly allied negro races still in Africa; or, were it attainable, a complete set of measurements of leading negroes in Liberia or Hayti, with the same Galtonian comparisons carefully made. Mr. Grant Allen certainly gave us a startling commentary on brain-action and degeneration combined, and working hand-in-hand—most to the left-hand, however—on the grandly-conceived, most artistically worked-out story of the Reverend Mr. Creedy! But, according to him, Mr. Creedy survived, and was, therefore, of the Fittest.

V.

In fact, the whole fallacy of one side of Mr. Darwin's teachings results from his lack of subtlety and true thought-power. He sees things too much alone, detached and simple, but they are not so, as in the passage above, from Professor Cope, is directly suggested. M. J. Massart, in association with M. Emile Vandervelde, has for years been working to establish, by instance, illustration, and comparison, that degeneration and reversion are not to be absolutely separated and put aside as though their laws were something apart, distinct from, and opposed to, any fully realised law of evolution. The two interact and are inseparable indeed. Degeneration (or reversion) and progress appear to him as the two sides or aspects of the same process of evolution—the one is not to be separated off and labelled as though it were a train running exactly in the opposite direction to the train advancing forward the other way on companion or parallel rails. No, they run together—it is a mixed train, both proceeding on one line. In "Parasitism," as in MM. Massart and Vandervelde's more important "Evolution by Atrophy," it is the same, and though, like Mr. Darwin, they are constantly taking a text from lower life to reveal less obvious laws of human and social development or evolution, they keep close and clear to their great and leading idea that all is one whole, moving in one direction. The atrophy of an organ means the inevitable building up and perfecting of another—the translation, say, of a reptilian fore-limb into the wing of a bird. This principle was more recognised in Lamarckism, and in new Lamarckism, than it is in Darwinism, new or old; and this principle will more and more re-emerge and triumph, because it points to a higher unity and design than Mr. Darwin really attained.

If Mr. Darwin had reached this idea there could not then have been the constant contest in his mind between the conceptions of "selecting," "preserving," and "killing off"; nor could there have been the salient and ever-recurring difficulty and contradiction about aborted organs; nor about many other matters more or less related to them. It comes back to the old point: Mr. Darwin was a great observer, experimentalist and classifier, but he was not a thinker, and whenever he essayed that *rôle* he only added to the difficulties that he himself saw and confessed, a sense of utter and inept incapacity, descending even to obtusity in the use of words, and contradictions and errors inevitably arising from it.

In the physical economy of man, the same results are working themselves out in the higher science of medicine. There, too, the germs of dissolution or disease begin to be demonstrated as possible agents in liberating, in conjunction with others, new forces for health, strength, life. It is a wonderful system of re-adjustments, compensations, transformations. We find Dr. A. M. Brown in his masterly "Animal Alkaloids" (p. 143), thus summarising Professor Armand Gautier's results on one great section.

"Is it not admissible for us to ask whether fever itself . . . . may not have for its physiological aim and object the destruction of these deleterious substances, which, under certain conditions, are formed, or form themselves so abundantly in the economy?"

This is at the basis of the new and more scientific medicine, as it is really at the basis of purely scientific thought; it recognises, as the other does, that processes are not simple but very complex, and that the business of scientific medicine is to master the excess in one of two opposing forces; and thus, by achieving true compensation and balance, restore health, activity, life. Death is but the result of failure to attain this; the very germ becomes too assertive and strong, and at that point there lies a necessity for readjustment as integral to the very basis of life, health, and propagation of the species; excess of one factor at a certain point is death. Exactly as that subtle and seer-like speculator on human life, its motives and perversities, Nathaniel Hawthorne, said: "Pain is but pleasure too strongly emphasised."

VI.

The persistency of the negro's dark skin and his prognathism is, however, no more remarkable than is the persistency of fair hair and fair skin on the part of races that have for ages on ages lived in tropical or very warm countries. As we write we read in effect these results in Mr. Keane's valuable and suggestive "Ethnology":—

> "The northern coast of Africa is still occupied by a fair-haired, blue-eyed race which has all the characteristics of the Red Kelt, and the traveller who finds himself among the Red Kabyles of Algeria may be pardoned for thinking that he has been suddenly transported to an Irish village. The Egyptian monuments show that this fair-haired race was settled on the African coast as far back as the dawn of history, and that it even extended into Palestine, where the Amorites of the Old Testament were similarly distinguished by blue eyes and blond hair. The cromlechs and other megalithic monuments which are so plentiful on either side of the Jordan, and extend along the North of Africa through Western Spain and France into England and Scotland, enable us to trace the line of march by which one of the prehistoric races who peopled our islands made their way from the Mediterranean. This was still in the Stone Age; the round-headed people of the round barrows had not yet arrived with their tools and weapons of bronze."

Mr. Keane roundly tells us that the primeval home of the Caucasic race was "Africa north of the Sudan. . . . ."

This assuredly is a somewhat startling assertion, since it raises the question as to how the white race could have permanently acquired its white skin. That man was originally dark, or at least yellow-skinned, is now generally admitted, and most theories in explanation have proceeded on the assumption that it took place in a cold and cloudy climate. But when the cradle of the white race is assigned to Northern Africa the problem becomes more complicated. True, the winters are severe in the mountain ranges where the modern Berber tribes mostly live, but even so the air is dry and the skies clear, while the summers are hot.

The Azaude of Central Africa, Captain Guy Burroughs tells us, have not at all the negro face. Their hair is curly, not woolly. Their mouth is not that of the negro at all ; some faces among them differ only in colour from that of the European Jew.[6] They are very clannish, and consider themselves superior to all those around them, and have no doubt been settled there for many centuries.

In Central Africa, again, there are other tribes—the Gallas especially—whose customs and traditions clearly point to Egypt, Syria, and other regions, in which fair complexioned races have certainly resided. Desmoulins gives an account ("Histoire Naturelle des Races Humaines," p. 168) of a fair-haired race which has for ages been established in one of the hottest regions of India, which, by avoiding intermarriage with the dark tribes round it, has preserved its original peculiarities during the long space of six hundred years. This is the Afghan tribe of the Rohillas, settled on the south of the Ganges since the accession of the Patan dynasty to the throne of Delhi in the thirteenth century. Isolation here does not promote variation. Dr. Shaw, in "Travels," p. 120, describes the inhabitants of the mountains of Auress, in Algeria, as being fair and ruddy, and their hair of a deep yellow.

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[6] "Land of the Pigmies," p. 62.

But however certain races may have originally acquired the white skin and blond hair, they retain these traits as persistently as the negro holds by his dark skin, prognathism, and wool instead of hair; and here, if there is mystery about the negro, there is also mystery about the blond races that have for long ages lived under eastern suns and yet have fair hair and fair skin.

Here, certainly, there is more tribute to Mr. Darwin's idea of slowness in the operation of condition to modify traits and structure than anywhere else in nature. But here we do not find Mr. Darwin enlightening us very deeply about the reasons; he is, on the whole, too intent on catching the points where superficial anatomical resemblances arise between the quadrumana, &c., and man, to celebrate so effectively as he might such vast apparent *distinctions* between man and all the lower creatures. Miss Buckland, at one place, dwells on the early migrations of human races, and gives good reasons for believing that they must have been extremely slow, like those of animals; but I am not aware of any thoroughly exhaustive treatise dealing satisfactorily with those great outstanding facts as regards the permanence of traits in the human species, in face of utmost changes of conditions; and a considerable study of the early Turanian race, representatives of which are still found at extreme points of separation from each other, in different climates and latitudes, as they have been for ages on ages, raises some doubts, indeed, as to whether Miss Buckland's general statement above applies on every side, and absolutely to them and their migrations. They do not name themselves Turanians, and the best etymology of that name is from the Sanscrit TURA, swift.

The writer of a remarkable article in the *Quarterly Review* for July, 1899, remarks, opposing the idea that temperature is a powerful element in deciding the distribution of mankind :—

"That temperature has no great importance in the distribution of mankind is proved by the fact that human races are not limited by isothermal lines, but by the physiographical ranges of continental areas." [7]

And on the special matter of modifications on the negroes in the United States, he goes on to say :—

"The negro in America, although of recent importation, has already changed somewhat in features and colour. The variation is as yet very slight, but then we must remember that it takes ages, not centuries, radically to modify the peculiar characteristics of a race, and that no change can be expected unless the new conditions of life be essentially different from those of the native environments." [8]

And yet in spite of this great persistency of type and structural mode in races of men, there can be no doubt that conditions are decisively operative in the production of them originally. Professor Huxley makes this very plain by a bold geographical line :—

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[7] P. 269.

[8] P. 270.

"Draw a line on a globe from the Gold Coast in Western Asia to the steppes of Tartary. At the southern and western end of that line there live the most dolichocephalic, prognathous, curly-haired, dark-skinned of men—the true negroes. At the northern and eastern end of the same line there live the most brachycephalic, orthognathous, straight-haired, yellow-skinned men—the Tartars and Calmucs. The two ends of this imaginary line are indeed, so to speak, ethnological antipodes. A line drawn at right angles, or nearly so, to this polar line through Europe and Southern Asia to Hindostan would give us a sort of equator around which round-headed, oval-headed, prognathous and orthognathous, fair and dark races—but none possessing the excessively marked characters of Calmuc or negro—group themselves. It is worthy of notice, too, that the regions of the antipodal races are antipodal in climate, the greatest contrast the world affords, perhaps, being that between the damp, hot, steaming alluvial coast plains of the West Coast of Africa and the arid, elevated steppes and plateaux of Central Asia bitterly cold in winter and as far from the sea as any part of the world can be.

"From Central Asia eastward to the Pacific Islands and sub-continents, on the one hand, and to America on the other, brachycephaly and orthognathism gradually diminish, and are replaced by dolichocephaly and prognathism, less, however, on the American continent (throughout the whole length of which a rounded type of skull prevails largely but not exclusively) than in the Pacific region, where at length on the Australasian Continent and in the adjacent islands the oblong skull, the projecting jaws, and the dark skin reappear, with so much departure in other respects from the negro type that ethnologists assign to these people the special title of negritos."[9]

But when we find such marked changes in a very short while on animals through being transported merely to France, not to speak of the West Coast of Africa, &c., it is not surely unallowable to suggest that science, and especially selection, should have more to say than it has yet said on the remarkable permanence of traits and points of structure in the human species.

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[9] "Man's Place in Nature," p. 153-4.

VII.

On page 2, introduction to "Domestication," I find Mr. Darwin writing :—

"If organic beings had not possessed an *inherent* tendency to vary, man could have done nothing by 'tamperings with nature' to cause variability" ; and then on page 8 we have: "On the principles here sketched out, there is *no innate or necessary tendency* in each being to its own advancement in the scale of organisation" ; and then he proceeds to argue that variation being "specialisation and differentiation of parts," tends to complexity, and that "natural selection, acting exclusively through the preservation of *profitable* modifications of structure," we may believe that, "on the whole, organisation advances. Still simple forms may remain *for ages* unaltered and unimproved." "Members of a high group *might even become*, and this *apparently* has occurred, fitted for simpler conditions of life ; and in this case, natural selection would tend to simplify, or degrade, the organisation, for complicated mechanism for simple actions would be useless, or even disadvantageous. Natural selection, too, *acts exclusively* through the preservation of *profitable* modifications of structure ; the *conditions of life* in each area generally becoming more and more complex from the increasing numbers of different forms which inhabit it."

Now (1) while "natural selection acts *exclusively* through the preservation of profitable modifications of structure," it will also act so as to simplify and degrade the organism; so that, logically, here there is no real distinction between "profitable modifications of structure" and "degradations of the organism"; so-called development and reversion are one, and cannot be distinguished or separated from each other. (2) Mr. Darwin asks what would it profit an infusorial animalcule, for instance, or an intestinal worm, to become highly organised? *Answer:* it would, in strictness, cease to be an infusorial animalcule or an intestinal worm, at the first step of the progress towards the assumed higher organisation; but, as we know, creatures advance from increase by fission merely through production of immense numbers of eggs, up to reduction of number and individual care for the progeny, and we must assume that there is not really the fixity absolute "for indefinite ages" implied in Mr. Darwin's "very simple forms fitted for very simple conditions of life, that for indefinite ages remains unaltered and unimproved." Mr. Darwin's own theory, on one side, implies, and cannot but imply, a continual and ceaseless ascent from this very condition, though, on the other hand, he maladroitly denies it. Life begins in a low and simple form, but the fact that countless higher forms of life exist demonstrate their emergence from it, else they could not be; and if there, indeed, is ascent, in Mr. Darwin's view of it; if, "on the whole, organisation advances," "innate tendency," alike in species and in individuals, there must be, or else some creative or directive power, wholly external to them, imposing upon them a force or law determining them in a definite direction against their "innate or necessary tendency." (3) Mr. Darwin speaks, as we have seen at page 290, of "innate variations," and then, at page 8 of introduction to "Domestication," declares there is no "*innate or necessary tendency* in each being to its own advancement in the scale of organisation," so that the "innate variations" of page 290 must be variations, not in the line of the creature's "own advancement in the scale of organisation"; that is, all the innate tendency is to variation in reversion or degeneracy. Is that what Mr. Darwin really means, if it is what Mr. Darwin logically says? No; it is not and cannot be; but that is the logical result of Mr. Darwin's own words.

(4) If there is "no innate or necessary tendency in each being to its own advancement in the scale of organisation," then clear it is, abundantly clear, that all variation in the direction of greater complexity, which is "advance in the scale of organisation," must have been in the teeth of innate or necessary tendency of some kind there; and then how does Mr. Darwin, how do his followers, reconcile these two things—a whole line of continuous advance upward in organisation in face of innate or necessary tendency on the

part of each being in a wholly different direction? That is what I should like to know—that is what I fail to find any satisfactory approach to meeting and conclusively and consistently setting forth or demonstrating in the works of Mr. Darwin. They are full of interesting facts gathered by study or observation from widely-separated fields; they are carefully put together, and sometimes with utmost effect either of fine comparison or of contrast; but when I come to the pure thinking part, then I am fain to say I never met with more clumsy infelicity of language, with more crass contradiction and incapacity.

(5) "If organic beings had not possessed an inherent tendency to vary" man could not in the least have worked artificial selection, and yet if there is no "*innate or necessary tendency* in each being to its own advancement," that is, to greater complexity of structure and function, from whence does Mr. Darwin derive the law of order, harmony, and development? It must be from something outward, outside wholly of the individuals who are presumed the subjects of change or advancement; and if there is a "tendency to vary" and yet "no innate or necessary tendency in each being to its own advancement," where, I ask, is the provision for the progressive development, or, on the whole, as he says, "organic advance?" I can see no provision for it in these statements I have dealt with; I can see in all this only a return on either of these two things—(1) some purely general and imagined entity called *nature*, to which, indeed,

despite his pretence to deal only with facts, Mr. Darwin turns back when it suits him—to nature, which is "only the *aggregate* action and *product* (!) of many natural laws, which laws again are only the ascertained sequence of events" ("Domestication," i. 6). But a law, or many laws, which is or are only the ascertained sequence of events "surely cannot be regarded as having any *products* outside that ascertained sequence"; therefore either Mr. Darwin's definition much needs mending or he has to revise his whole view of nature, which logically as it stands with him here becomes a mere *Deus ex machinâ*—God or devil, what you will. Or (2) he *must* admit the determining power of some influence or force outside events, the "ascertained sequence," of which, mark you, is nature, for as there is no "innate or necessary tendency in each being to its own advancement in the scale of organisation," all the advancement that is made must be made by some influence or power superior to and controlling that, else on these premises there is no law or order at all, and things develop in opposition to their own "innate or necessary tendency," and you are back on the old theological ground, though the name of God is Nature.

"The *conditions* of life" besides have become more and more complex on each area from the increasing numbers of different forms which inhabit it; and if this "complexity of conditions" becomes only more and more marked, what of the deliverance that there is "no innate or necessary tendency in each being to its own advancement on the scale of organisation?" Taken strictly this means, and can only mean, that the apparent order in the world advances, or is maintained by arbitrary power outside it all, or by something like CHANCE; and here I have the only sufficient explanation I can get of the tendency of these gentlemen, one and all, freely to use such words as "chance," "happened," "fortuitous," &c., &c., and then try to put some novel interpretation on them when they are called to book about them. Dr. A. Russel Wallace is very anxious to get rid of this idea of chance, which legitimately or otherwise has invaded the field, and gives him great trouble and concern, but not with complete success either; and Mr. Grant Allen goes freely on, on with his "*rough and ready*," "*rough and ready fashion*" of nature, which is chance; and Professor E. Ray Lankester exults in his "non-significant *fortuitous* congenital variations."

And then, though Mr. Darwin, as Mr. Wollaston Hutton tells, in the beginning of 1867 was led to change his view on a most important point by reading Professor Fleeming Jenkin's article in the *North British Review;* notwithstanding all this, in 1868 he publishes "Domestication," with a preface or introduction, referring to and basing on "Origin of Species" without the remotest hint of his having changed his opinions in any important particular, and, as it appears, is then and still standing by the "Origin" absolutely and unqualifiedly. Was that honest, was that conscientious scientific work, was that what we should have expected from the frank and conscientious Mr. Darwin? I shall be glad of Mr. Hutton's answer to this.

VIII.

In my honest and persistent endeavour to follow the developments of Darwinism, I come on the most unexpected and characteristic attempts either to compromise a bit and to "run with the hare and hunt with the hounds," as the saying is, or else to confuse and mislead those who accept, too easily, the deliverances of favourite journals, or else the writers are themselves overborne by some vague and inconclusive and illogical vagaries of their own. Read the following passage, in which it is abundantly clear that the writer would fain preserve some clear ground for Darwinism, while yet he cannot escape the powerful hand which the Lamarckians or neo-Lamarckians have put forth upon him—rather to confuse and perplex him, I much fear:—

"The meaning of 'acquired characters' is usually, as by M. J. Constantin in 'Les Végétaux et les Milieux Cosmiques (Adaptation, Evolution),' not rigidly defined nor separated from latent possibilities in the organism, which the environment is able to emphasise simply by providing that stimulus which assures their positive appearance. Some of these variations, responsive to the external requirements, are certainly very difficult of explanation on the doctrine of selection, but the opponents of this theory sometimes overlook the fact that, in the first place, it is not in the least necessary to assume that variations will be *slight;* they are often, on the contrary, in the case of specially plastic individuals, very extensive, when these are subjected to a change of environment."[10]

In the light of what we have said it will be clearly on the minds of our readers that it is the Darwinians who are eager to ignore the possibilities of anything but very slight variations or modifications—modifications *very slight,* non-significant, and slow; and that the "opponents," who are here credited with *that* weakness, are the very men who have fought, tooth and nail, against the acceptance of that dictum. According to this writer, they overlook a fact on which, in part, they have actually built many of their arguments against selection. They certainly have never assumed that variations *must be* slight; they have, on the contrary, held and argued and presented instance on instance, where, "in the case of especially plastic individuals," very extensive variations resulted when these were subjected to a change of environment. More or less, all these men hold that Mr. Herbert Spencer is right when he defines selection as nothing but "another name for *organisms regarded as adaptations to environment or to external*

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[10] J. B. F., in *Nature,* August 31st, 1899.

conditions." That may go too far in the expressions of some as regards setting aside constitution or organisation of the plant or animal dealt with—clear recognition of " innate tendency to vary " or " latent possibilities " ; but such charge cannot surely be raised against Professor Cope and many of those whose names rise to the mind on the mention of his.

"*Latent possibilities* in the organism," what does that mean? what does it include? what does it exclude? Is the " tendency to vary " of Mr. Darwin, noted and dwelt on by us in last section, something absolutely different from, and opposed to, his deliverance at page 8, " Domestication," vol. i., that " on the principle here sketched out, there is *no innate or necessary tendency in each being to its own advancement in the scale of organisation?*" If this is so, from the Darwinian ground, " latent possibilities " can only be to variation in the direction of *non*-advancement in the "scale of organisation" ; that is, to degradation or reversion. What, then, are this writer's " latent possibilities " ? Mr. Darwin is not a satisfactory writer on such themes as these—to those that think and wish to be clear and make clear ; the more he is followed and analysed, the more he perplexes, the less he satisfies. I ask this writer, and beseech him to answer me : do his " latent possibilities " lie in the line of Mr. Darwin's " inherent tendency to vary," or do they in any way illustrate " the principles here sketched out (page 8 of " Domestication," vol. i.) that there is no *innate or necessary tendency* in each being to its own advancement in the scale of organisation"? When he has clearly and exhaustively answered that in *Nature*, or otherwise, then we can understand his deliverance and attach fitting importance to it; then, and not till then, can we reconcile his notion of "latent possibilities" with the very extensive variations "specially plastic individuals" show when they are subjected to "a change of environment." These extensive variations, again, are they in any degree illustrative of Mr. Darwin's "*no* innate or necessary tendency in each being to its own advancement in the scale of organisation"? in other words, are the "specially plastic individuals" plastic only in "an inherent tendency to vary," but have no innate or necessary tendency to advancement in the scale of organisation? I anxiously await for his reply, and, meanwhile, venture to assure him that *slight*, *very slight*, is the Darwinian craze, and certainly not that of those on whom he would fain father it, to make out his view easily. But I anxiously wait his reply, and sincerely hope the editor of *Nature* will not forget to keep him well up to it.

IX.

It is a great thing with men like Mr. Hutton, following in the line latterly suggested by Darwin, that selection is really "killing off the weak"—unqualified killing off—to say that selection is not selection at all, but *elimination*—that nature "red in tooth and claw" only works through selection on the downward grade with the all too curious result that something else works upwards as it were by chance, as Mr. Allen says, so that we have this funny position, that selection is not only a misnomer, but logically a fraud, "shifty," I should call it; or, if there is in animals "an innate tendency to vary," does it not (1) in the whale, too, distinctly show it is not there operative, though (2) it also proves that there is no innate and necessary tendency to its own advance in the scale of organisation, though, indeed, it is scarcely consistent that Darwinism should here so completely have it both ways—both ways, that is, an apparent and sham advance on one side and really a retrogression on the other, exhibiting a tendency to vary and yet having and showing no "innate and necessary tendency to its own advancement in the scale of organisation."

If selection, as presented to us by Darwin, only works by elimination, why do we find characters stamped on certain creatures through the milleniums with what are clearly not so advantageous organs as might be? There handy, as we have just referred to it, is the whale. Clearly, lungs are not so advantageous to it as gills would be. Look at the business of suckling, surely a point that bears very directly on use and the preservation of the species; yet the whales go rolling round and round as the young one sucks so as to get turns of air. Why has not nature through selection, which is "elimination" or "killing off," eliminated these lungs and developed gills as she has done, and done so easily and quickly, in so many other and less striking cases? That is one of the questions for Mr. Hutton to address himself to and settle. Nor is it easy to see how *in this case* the fact of persistency in lungs—internal organs—can be classed under the *rôle* of "recognition marks" as some —and those leaders in Darwinism, too—have classed so much, while as for checks on intercrossing these seem, so far as this is concerned, to have been but too effective in this special instance.

And then the "recognition marks" theory certainly wants some further qualification in view, say, of the remarkable variations in stripes and crossings of donkeys, not to speak of Professor Cossar Ewart's zebra hybrids, and to all these hybrids which are proved to be *sterile*, and in which the markings are frequently exaggerated and extreme. In that case there can be no need for "recognition marks" from one very special point of view, that is, as useful to progeny—the first most essential and vital use of "recognition marks." There is a saving-clause in the idea of these "recognition marks" being a powerful check on intercrossing; but this subject wants a great deal more investigation of the close, persistent, and thorough kind than it has yet got.

Mr. Darwin, as we have seen, says that "*simple* forms may remain *for ages* unaltered or unimproved." But other than what he means by "simple forms" here remain for ages "unaltered and unimproved." We hark back here on the whale. That is not a simple form, yet, from the far-back tertiary period, it seems to have remained *in statu quo*. On Mr. Darwin's principles, this ought not so to be. Stagnancy absolute, according to him, is only for simple forms. "On the whole, organisation advances." On the whole, as regards the whale, with lungs instead of gills, with which it could breathe freely under water, organisation, so far, does not advance, does not adapt itself; nor do we, as yet, see any tendency whatever in whales to slowly, slowly, develop gills. *Elimination*, killing off! Yes, but here Nature does not eliminate; selection, in the true and original sense of it, does not act. I go no further for instances, as I wish to press this point; but there are instances many to quote still, which I will here hold in reserve till I have got from Dr. Russel Wallace, or Mr. Grant Allen, or Mr. Hutton (Captain, or Professor, or both in one) satisfactory explanations on this pressing point. Here, clearly, a weakness, a disadvantage, a non-adaptation has not been eliminated, and yet whales survive, would indeed increase vastly were it not for harpoons—against which, by-the-by, gills would be vastly advantageous—and natural selection, now as natural elimination, or killing off, has not had its perfect work: the whales survive, and, with non-adapted organs, are yet truly the fittest.

APPENDIX.

I.

Food, Fat, and Fertility.

REFERRING, once again, to Mr. Darwin's perversion, that "domestic animals that have regular and *copious* supply of food without the trouble of searching for it are more fertile than their wild congeners," I may tell the results of a glance through certain papers that have to do with stock-breeding. The first point that catches my eye is a statement of the independence, in so far, on water on the part of fattened cows, in the last stage particularly, compared with milch cows, who are very much more dependent on water. "Fattening cattle require very much less water to enable them properly to digest their food than do milch cows. It is well known that, weight for weight, a dairy cow will consume considerably more food than will a fattening beast." Discrimination as to food is very essential, too; elements much called for, in the one case, may be even injurious in the other, or anyway, not go to secure the object the rearer has in view. Wolff found that if less than $2\frac{1}{2}$ lbs. of digestible proteids were fed to ordinary sized milch cows, the animals shrunk appreciably in their milk yields.

The next note I read is this :—

"The ideal show Jersey cow of the present day is very little better than a mass of bones covered over with skin ; the flesh is quite a negative quantity. Some of the best specimens of the breed at present before the public are just the very opposite of what a beef-producing beast should be."

"The copious supply of food," therefore, may be the right thing if your intention is fattening for market, but it is a very different matter if you want milk and, in due season, calves, and vigorous calves.

Thus, though the heifer may actually consume more food, considering it weight for weight, than a fattening animal; the very object is still to prevent fattening as alien to fertility.

A friend of my own had a shorthorn heifer, which, being made a pet by the ladies, was spoilt and over-fed—had, in fact, enjoyed "a regular and copious supply of varied food without the labour of searching for it." Though put to good bulls no fewer than nine times, there was no result. At last my friend tired of this, turned the cow out on an odd, not very rich, strip of pasture, and kept her there. A neighbour had put a shorthorn bull into a near field ; he broke over the enclosures, and coupled with the heifer, and the result was a bull-calf. This same friend declares that, as to sows, in view of breeding, one thing is to keep them in good condition, and yet without fat; as if fat is laid on, especially with the better breeds, not only are few pigs got, but those that are got run greater risks of being overlaid.

II.

Here is a passage which opens up a most peculiar and interesting line of suggestion:—

"No satisfactory explanation has yet been given why it is that freemartins so very seldom breed. A freemartin, as is well known, is a heifer which is born as a twin with a bull-calf. The peculiar part of it is that the bull in such cases very seldom proves infertile, whereas the heifers have been known to breed only in very isolated cases. The commonly accepted explanation is that the male fœtus, being the more vigorous of the two, develops more rapidly than the female, and thus adversely affects the proper growth of the reproductive organs of the latter."[1]

If this presumption is correct, if, indeed, there is anything whatever in it, you have here an illustration of Haeckel's principle set forth at p. 168. Here, even in the ante-natal condition, the fœtus of the male in its greater capacity of development is assumed to draw from the fœtus of the female certain powers, the results remaining through the entire life of the two twins so far as affects fertility or heredity.

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[1] *Farmers' Gazette*, August 19th, 1899.

Has there been any careful inquiry into the analogous cases in the life of other species? What of sheep? What of cattle? What even of human beings? Has there been any attempt to show how it stands as to fertility or sterility in male twin and female twin? Here is a subject for an investigator like Mr. Galton, and it were to be wished that he or some one after his style should undertake it, as in one branch it would do not a little to modify the force of Haeckel's words to the effect that "this remarkable phenomenon is still very obscure, and has not yet been properly investigated."

III.

With reference to the report on Salmonidæ to the Scottish Fishery Board by a committee of the Edinburgh College of Physicians, spoken of at p. 27, we find that good authority, Mr. Darcy J. Hamilton, writing with the main facts of that report clearly in his mind :—

"In all probability the migratory Salmonidæ of the American Continent are the same; indeed, the fruitlessness of angling for salmon in some of the rivers of the Pacific coast would seem to show that even the impulse to seize food is deficient, although in this connection it is sometimes necessary to except the land-locked salmon."[1]

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[1] *Farmers' Gazette*, August 26th, 1899.

IV.

Mr. W. B. Tegetmeier, in *The Field*, September 2nd, 1899, is of opinion that crosses between male Reeves' pheasants and common pheasant hens are sterile. The Reeves from their size drive away the ordinary males and produce their sterile crosses.

V.

Salt has been found to have an injurious effect upon pregnant ewes kept on land so treated.

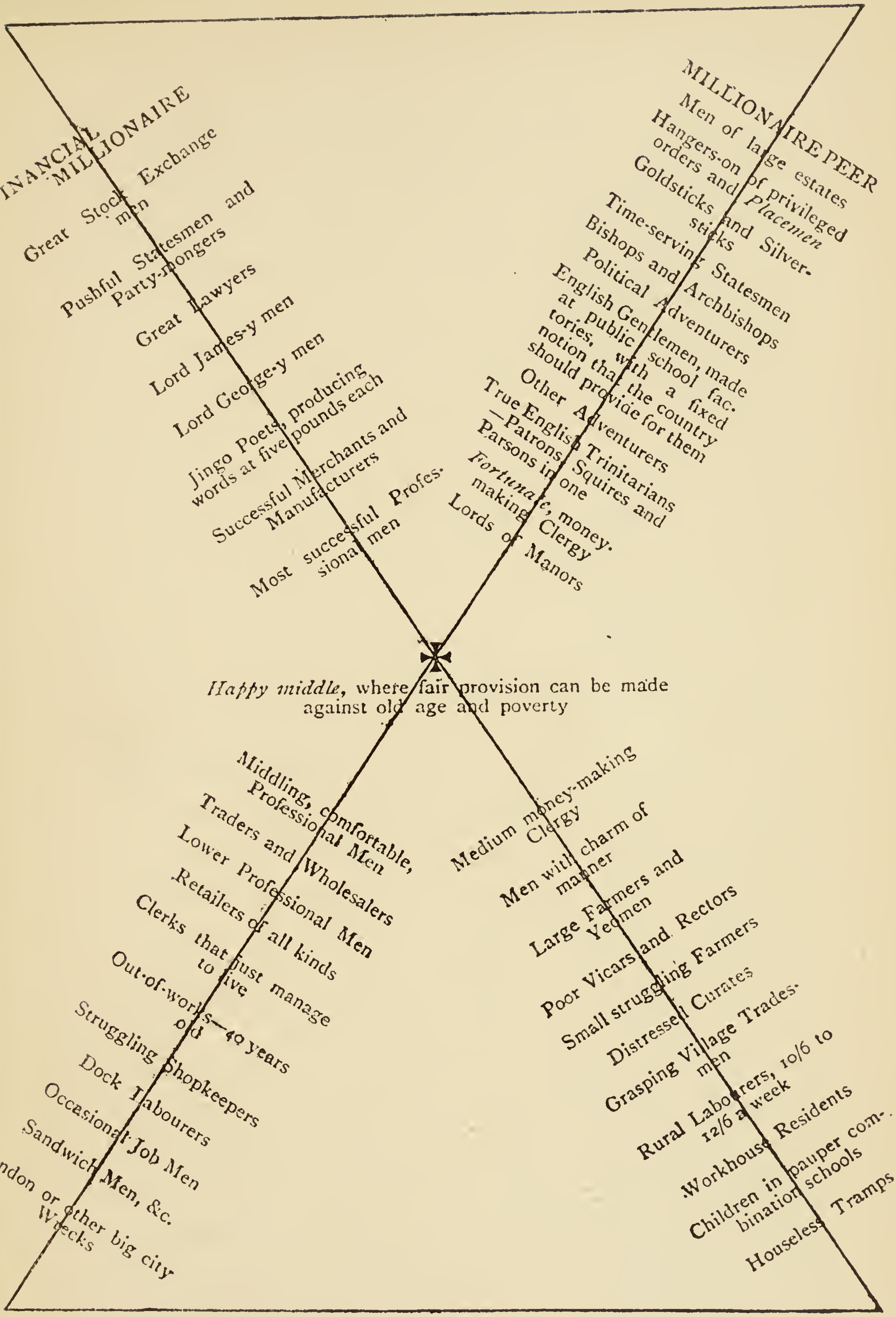
VI.

With reference to our remarks on Lord Rosebery's presumed equality of life, as between rich and poor, where from first to last he confounds mere means of gratification of appetites with ends or final results, and thus shows a very plausible smartness, but little, little philosophy—at all events of the kind we admire in Marcus Aurelius, or even in Æsop or Epictetus, not to speak of later utterances, such as those of Tolstoi, or Kant, or Fichte, or Carlyle at his best, among prose writers ; and of Wordsworth or of Matthew Arnold, among later poets—let us say that we cannot really withdraw anything of what we have there said. Lord Rosebery's words are, after all, nothing but a shallow apology for the inequality of artificial civilised life ; so dreadful, so tragic, on the one side, by reason of want, overtoil, and slow starvation ; so dreadful and tragic, on the other side, simply by reason of the tendency to throw out of sight the real sources of these evils, so interconnected and inseparable. Wealth drawn constantly, and constantly accumulating, in few hands, without real effort and exercise of faculty on their part to secure it; toil and struggle of millions in bad air, with bad light, bad water, bad food and adulterated food, and slow starvation and poisoning, not only allowed, but patronised by a Government which so profits by it that, in view of revenue, it cannot do without it. The one is but the correlative of the other; there is no explaining it away; the two facts, regarded as poles of the social or economic condition, if you will, are inseparable, even though, as we have said, final results may be the same for both ends.

But assuredly Lord Rosebery did not reach so near to the root-principle of the problem as Mr. Cornelius Vanderbilt, the American millionaire, did, when, in misery, pain, and self-hate (the Nessus shirt he was doomed to wear), this confession was wrung out of him :—

"My health is shattered, and all the money I possess cannot restore it. I cannot even drive one of my fine horses. It is painful for me to sit down. My only possible exercise is for me to walk down the avenue. I receive threatening letters daily, and my nerves are so unstrung that I am constantly afraid that some assassin will waylay me. I am overrun with people who want to get money. *I am the most* *wretched man in New York*, and I tell you that *after a person has accumulated enough to secure him against poverty, and gratify his reasonable wants, every dollar in addition is a burden, and weighs him down.*"

There, though it comes as a wail of individual woe, is some approach to a declaration, direct from experience, of the final, essential equality in the seeming inequality of circumstances of rich and poor; there the confession touches the heart of life and human experience and possibility. "*I am the most wretched man in New York.*" The irony of that in a millionaire does not breed true pity, but a kind of contempt, as of one who has had greatest chances in life and most utterly failed to lay hold of them, and then whines at life's ironical inequalities and equalities! Lord Rosebery does not quite say that. No; he says the rich man is not so much more happy than the poor, and tries to give impression of fair balance. To make that proposition intelligible, he wants to define which rich and which poor he means. If you take a line and carry it through the various grades or positions of society, from the millionaire peer, at the one upper corner, or from the speculative financial millionaire, at the other, right through the innumerable layers down to the poor wretch that must seek nightly repose on the Thames Embankment or under some bridge arch, thus you have a peculiar diagram :—


FINANCIAL MILLIONAIRE
Great Stock Exchange men
Pushful Statesmen and Party-mongers
Great Lawyers
Lord James-y men
Lord George-y men
Jingo Poets, producing words at five pounds each
Successful Merchants and Manufacturers
Most successful Professional men
MILLIONAIRE PEER
Men of large estates
Hangers-on of privileged orders and *Placemen*
Goldsticks and Silver-sticks
Time-serving Statesmen
Bishops and Archbishops
Political Adventurers
English Gentlemen, made at public school factories, with a fixed notion that the country should provide for them
Other Adventurers
True English Trinitarians —Patrons, Squires and Parsons in one
Fortunate, money-making Clergy
Lords of Manors
Happy middle, where fair provision can be made against old age and poverty
Middling, comfortable, Professional Men
Traders and Wholesalers
Lower Professional Men
Retailers of all kinds
Clerks that just manage to live
Out-of-works—40 years old
Struggling Shopkeepers
Dock Labourers
Occasional Job Men
Sandwich Men, &c.
London or other big city Wrecks
Medium money-making Clergy
Men with charm of manner
Large Farmers and Yeomen
Poor Vicars and Rectors
Small struggling Farmers
Distressed Curates
Grasping Village Tradesmen
Rural Labourers, 10/6 to 12/6 a week
Workhouse Residents
Children in pauper combination schools
Houseless Tramps

Two pyramids, the upper inverted, point of junction at the "happy middle."

Surely, surely, looking at top and bottom and down these lines, Vanderbilt could not be after all "the most wretched man in New York." He had all he wanted, had secured all he had worked for. With houseless tramps at the end of the one line and city wrecks at the other, who have lost all, most of which he or men like him have got. No, it was a delusion; Vanderbilt could not be so miserable as they—unless, indeed, he was far more philosophic than Lord Rosebery, and then perceived, ah! clearly perceived, that he had by necessity of the case actively aided in creating these houseless tramps and city wrecks. Then, if so, he might well be "the most wretched man in New York." Otherwise his proclamation of his misery is blatant and hypocritical egotism of the lowest kind; also like that of Lord Rosebery, which takes credit only for a sort of superior delicacy and fineness, don't you see, after the manner of the hot-house plant. But then, if Rosebery's supposition is correct, with weaklings from hot-house delicacy at the top, and weaklings from incessant toil, bad food, bad houses, lack of light and air, and even of good water at the bottom, how are you to fare in the event of a great struggle in Europe for defence, for endurance, for grit and grist? That is a matter of high national concern, too, if Lord Rosebery's words have the general application he fain would give them, so as to secure a strong army, a strong anything, indeed! Do Lord Rosebery's words have any bearing on the wretched officering of the British army with the constant refrain "some one had blundered," and the lack of fit recruits even now getting to such a pitch that conscription is threatened and coming? Rudyard Kipling is now the *great* poet of England. He may sing as grandly as Milton did or Tennyson, but clear it is the possible Atkinses don't dance to his piping, yet so great is he that Mr. MacDonald is more "as his uncle" than anything else, president of Methodist Conference though he be. See Lobb's *Christian Age*. I believe the things are vitally connected, and therefore I so write.

Serious, very serious, it all is, as characteristic of English compromise, complacency, and easy compliance and vulgar self-conceit; funny it is, inexpressibly funny, to the detached and disinterested independent observer, and so rich in pure Christian *association* and nutriment for all the higher forces and aspirations in man is the following above referred to that we feel we *must* here give it *in extenso*, so rich a treat it is. It occurs in a leading article under the heading—

"THE REV. FREDERIC W. MACDONALD.—Mr. MacDonald not only attracts attention by his own strong and commanding personality, but also, and *this in a larger measure to the outside world*, by his relationship to Rudyard Kipling, the celebrated novelist, who is his nephew."[1]

Having been concerned with Darwinism, as we have been, it may be permissible for us to ask—Is this the result of natural selection or of artificial, or of reversion to ancient type, or what; or does it illustrate anything of that kind of colonial and associate parasitism which, as we know, goes for so much in certain planes of life?

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[1] *Christian Age*, July 26th, 1899.

VII.

Quarterly Reviews and Darwinian Doctrine.

The writer in the *Quarterly Review*, July, 1899, puts the axe to the root of Mr. Darwin's dictum that there is no innate tendency in animals to advancement in improved organisation. Where he deals with the transmission of acquired characters the evidence he finds to be overwhelming, and he conclusively demonstrates that it is not disease or injury that is transmitted, but rather only greater power of resistance and of repair. This he supports by reference to experiences in other climates, and dwells on the fact that the new theory of disease as due to bacilli is absolutely in favour of this principle. The whole argument, with its weighty facts and figures, is distinctly in face of Mr. Darwin's dictum. He cites the cases of rats, &c., to disprove the theory that each species occupies those districts and surroundings best suited to its life. Thus he secures "latent possibility" or "tendency to advance in line of organisation," though conditions are in his opinion absolute as to these having effect. He puts it:—

"By subjecting plants and animals to artificial conditions we can induce them to assume structures characteristic of plants and animals which normally grow under such conditions; and thus we get an experimental proof of the relation of structure and colour to environment."[1]

To show the extraordinary effects of food in modifying function and structure, he tells the case of a gull in the Shetland Islands, which twice every year changes the structure of its stomach according to its food, which is grain in summer and fish in winter.[2]

And as to "uniformity of condition," or anything like it, he is absolutely with me in this that any assumption of that kind is in face of a conclusive body of facts. He writes:—

"We know now that the surface of the earth has continually changed. Land has sunk beneath the ocean, fresh land has risen up from it destitute of land life; mountains have been elevated, altered, levelled; the physical conditions of large districts have been modified again and again. Organic life has, therefore, been subject to continuous displacement and alteration. But, even independently of topographical changes, we know that plants and animals are continually altering their areas of distribution in the daily struggle for life."[3]

Certainly, Mr. Darwin's uniformity of conditions can have no quarter from this writer; he recognises, as I do, "*continuous* displacement and alteration."

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[1] P. 272. [2] P. 278. [3] P. 282.

VIII.

With reference to our remarks at p. 31, to the effect that conditions of the most express kind come into play with the very fact of copulation, and because of the very sensitiveness of the sexual parts answering to the slightest modification of any other part or parts of the system, we may here set alongside these the remark of Professor A. Sedgwick in his Introductory Address to Zoological Section at the meeting of the British Association at Dover, in 1899, that "whereas the offspring resulting from asexual reproduction as a rule exactly resemble the parent, they are always different from the parent in sexual reproduction. . . I merely assert that whereas asexual reproduction is on the whole characterised by identity between the offspring and the parent, sexual reproduction is always characterised by differences more or less marked between the two."

Conditions, according to Professor Sedgwick, are the stimuli to which constitutional latencies or predispositions answer; without the stimuli the latencies or predispositions will not be excited into activity. The conditions that come into play with the very fact of copulation, arising for one thing, from the effects of certain recondite changes of condition upon the sexual organs, assures not only temporary but permanent effects upon the progeny — sometimes slight and more inscrutable, sometimes bolder and more unmistakable; but in the very facts of complexity in the union of two germs to produce a new individual you have assurance of difference and of variation.

Here we have from one side illustration—unconscious illustration so far, it may be—of the far-seeing words of Haeckel quoted at p. 29:

"Every change in the sexual organs reacts on the rest of the body; so, on the other hand, every important change in another part of the body must necessarily react on the sexual organs. This reaction will, however, generally only become perceptible on the formation of offspring, which arise out of the changed generative parts."[1]

IX.

Just as this volume is being finished at press I read the following in *The Daily Chronicle* of October 4th, 1899:

> "How to live on a penny a week—that is the problem which seems to have been solved by a woman at Marlow, if the report laid before the guardians at Chipping Wycombe, Bucks, is in any way correct. Her method was to spend the penny on a loaf, to cut off a slice a day, and to make her meal off that. This poor woman takes a place beside Carlyle's classic figure of the widow gathering her nettles. She is the type of a large class of poor creatures who do not starve outright, and create a case for the coroner, but silently and uncomplainingly perish from gradual weakness. It is useless to point to the workhouse: for that is the very thought that silences them. They foresee that pharisaic index-finger pointing to the door where

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[1] "History of Creation," i., 247 : *see also* i., p. 330.

hope ends. There is no remedy for such cases, except a more humane Poor-law, and some provision for old age, given with grace, so that it may be accepted without shame."

This is proof of how many honest, respectable people are being starved to death in this country at the end of this 19th century. Lord Rosebery's easy-going, nice way of comparing poor and rich does not go quite wide enough to include such cases as that, or to see them the least in relation to his fine philosophy. But they exist—cases of starvation to death, whilst the "too much" and "too many" are playing their havoc too—top and bottom united, to proclaim an inequality of means as to appetite, a great equality as to ends attained!

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